

**Robert E. Grant (1793-1874): The Science and
Social Predicament of a Pre-Darwinian Transmutationist,
Including a Study of Richard Owen's Response to the
Threat Posed by Grant's Transformism**

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Abstract

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R. E. Grant's materialist Lamarckism was sustained by the London radical movement in the 1830s. Its ideological value to doctrinaire democrats and especially T. Wakley is discussed. Grant's Geoffroyan morphology is historiographically important, enabling us to reinterpret the transition from Cuvierian functionalism to anti-teleological structuralism. The migration during the later 1830s and 1840s from radical transformist Geoffroyism to a bourgeois naturalistic archetypalism and eventually to Owen's Peelite morphology is considered from a contemporary political standpoint. The complex social and economic roots of Grant's decline are considered, particularly the adverse reaction of the Tory managers of the learned societies. Owen provides a paradigm of the Anglican response to the democratic challenge, and is treated from the conservative romantic perspective of the besieged College of Surgeons. Owen's stratagems to discredit Lamarckism are detailed, and it is concluded that his anti-transformist ideology had a positive heuristic value, leading to his recognition of the value of von Baerian embryology. The thesis employs models derived from the sociology of knowledge and attempts to interpret the changing structure of morphology in terms of social and political context.

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Chapter 1

Introduction: Statement of the Thesis and Some Historiographical Problems

Turning... to England itself and the British Empire, in its ‘relations’ to Science, It is a matter of just pride & pleasure to feel that she maintains her fair & high position among the Nations of Europe, in the achievements of the peaceful conquest of the regions of Truth. In that relating to life, & the conditions of life, which spread far & wide beyond our presentation Great Britain is known by the Names of Robert Edmond Grant, of Buckland, of Sedgwick, Murchison, Lyell, Darwin, who, with the Names of Note of my Successors in your Hunterian chair, have led the way to original discovery & to annexation of fresh territory to the Empire of the known.

Richard Owen, by now an eminent Gladstonian Liberal, speaking at an anniversary dinner in the Hunterian Museum, possibly in 1883 (1).

There is multiple irony in Owen’s generosity. Darwinian scholars might look with a jaundiced eye at Owen’s belated acknowledgment of Darwin; they must certainly wonder at such imperial praise for T. H. Huxley and his Christian protégé W. H. Flower (Owen’s successors in Lincoln’s Inn and protagonists in the 1859 debate). But historians, used to locating Owen and Huxley in a Darwinian context, might miss a further irony, equally unexpected but considerably more instructive: one that reveals a social episode barely touched on in older Whiggish accounts, and only beginning to be recognized in modern revisionist historiography (2).

Owen as an old man was casting his mind back half a

century, to the radical thirties – to a time when his democratic professional rival, Robert E. Grant (1793-1874), was defending a secular, transformist morphology; defending it, moreover against the counteractive social and scientific stratagems of the gentlemen of London’s learned societies and chartered corporations. The irony is not only that Owen in the Victorian ‘High Noon’ should rank Grant, already by then a forgotten man, with the elite founders of the science, forgetting his own former censure, but that the social lines of the debate should have become so obviously blurred. Grant, the poor anti-Oxbridge radical, was now incongruously ranked alongside upstanding Tories and wealthy Whigs like Buckland, Murchison, Sedgwick and Lyell: those who, in the years of revolutionary upheaval, worked to undermine his destabilizing science. The ultimate irony is that foremost among the respectable savants who recognized Grant’s political threat was Owen. In the 1830s he was himself a socially-aspiring morphologist, employed in the elitist College of Surgeons – an Anglican savant lionized by Oxbridge dons, friend of influential Tories, and patronized by Peelites in high office.

The lines of *this* debate have barely begun to be drawn by historians. Perhaps this is not surprising, with Owen often accorded no more than a reactionary role even in the Darwinian context. Only recently, as the discipline professionalizes, have younger historians of science with no evolutionary axe to grind begun to tackle Owen’s morphology and palaeontology from a contemporary perspective (3). Oddly

Grant remains almost completely unknown. He has generated no secondary literature and there exists no historiographical foundation to build on. So here I have had to start almost from scratch, resurrecting the London Lamarckian using the social and political context to produce a three-dimensional picture. How ‘Grantism’ as a radical doctrine was expunged in its day is a story that is central to this thesis, and I will suggest that it waxed and waned with the doctrinaire radicalism which sustained it. Respectable reformist science eventually carried the day, mirroring the success of Peelite compromise in the political arena. And as it did so Owen’s archetypal morphology superseded Grant’s science in the hungry forties. Why Grant himself failed to be reinstated in the new pantheon subsequently erected by Darwin’s disciples is a different story, which I will revert to.

Extreme interest attaches to the 1830s debate precisely because the political lines between the scientific factions *were* so clearly drawn (in large measure reflecting the radicals’ working-class demands and support for the “commonalty” in the corporations). The professionalization model that I employed in *Archetypes and Ancestors* to characterize the Darwinian debate cuts across class, doctrinal, and political boundaries to produce a picture of immense social complexity (4). Although my model in this thesis will undoubtedly require revision, it seems true as a first approximation that we can characterize the Lamarckian

and his *Lancet* allies as doctrinaire, anti-monopolist radicals: a vociferous, minority group which, in the House or out, championed the cause of labour and democracy. As the thesis proceeds we will come to appreciate how Grant's own materialistic morphology rested on this radical infrastructure as an expression of anti-Anglican, anti-aristocratic sentiment destructive of all privilege and nepotism. This social approach has a number of advantages. By exploring the social and political undercurrents, as Steven Shapin has shown in his study of the Edinburgh phrenologist reformers (5), we can understand the political *uses* and as a result offer a contextual explanation of the *structure* of Grant's science. A study of radical science is long overdue. The landed savants, the oligarchs of the London societies (6) and members of Cannon's 'Cambridge Network' (7), have been extensively studied; Jack Morrell and Arnold Thackray have detailed the way in which these *Gentlemen of Science* created a cultural resource as a recipe for social stability, i.e. to neutralize the radical-Chartist democratic threat, limit reform, and preserve Anglican hegemony (8). Yet the radical protagonists of those elite savants scarcely make an appearance; they exist as social shadows cast by actors standing off stage. While one gets a wonderful insight into the stratagems devised to preserve their class supremacy, one gets no feeling for the scientific doctrines that proved so terrifying. Nor, therefore, is there any understanding of the relationship between political radicalism and reformist science. Indeed, to my knowledge, there has never been an

investigation of the relationship between radicalism and its Geoffroyan scientific cutting edge. By understanding Grant's materialistic morphology and political extremism we can recast science history into a social form, obeying Morris Berman's injunction to interpret scientific change in terms of familiar categories like ideology, class conflict, and social status

Explaining Grant's Decline

I shall be very curious to know what has passed about Grant ... & what new eccentricity has been perpetrated by that shadow of a reputation.

Edward Forbes gossiping to T. H. Huxley in November 1852 (10).

By contextualizing Grant's science we can begin to explain his scientific decline in the 1840s, a time of success for Peelite compromise which gradually rendered radicalism socially irrelevant. But this political context is only the framework on which to hang an explanation - it is not the explanation itself. This must be at once more encompassing and more specific: it must break the issue down into recognizable mediating levels – identifying institutional ideologies, the effects of social conditioning in a specific locus (Grant in the materialist, Plinian atmosphere of Edinburgh, and Owen in the Coleridgean confines of Lincoln's Inn). It must branch out to encompass the respective roles of Presbyterian evangelism (particularly important for

determining the structure of Grant’s Calvinistic naturalism) and Oxbridge Anglicanism. And it must be sensitive to professional rivalry in a constricted, badly-paying field, and to personal idiosyncrasies and contemporary mores.

In short, as the radical threat to Owen’s Anglican morphology was multi-dimensional, and mediated at institutional, political, and doctrinal levels, so Grant’s rise and fall was itself a complex affair. This, then, is not a biography of Grant – the loss of his personal letters must preclude such an approach. Nor, for obvious contextual reasons, do I attempt to cover anything more than a short period, roughly 1826-41 – the years of heightened radical agitation and social stress. Indeed I concentrate on the turbulent period between the passing of the Reform Bill (1832) and the democratic restructuring of the corporations (notably the College of Surgeons, where Council elections were finally guaranteed by its new charter of 1843) and the anti-aristocratic reform of the societies (e.g. the Royal Society). It is a study of the social meaning of radical science, and an *institutional* explanation of the failure of Lamarckism and extreme Geoffroyanism to transfer successfully to the capital. It is also, in consequence, a study of a radical’s decline: an attempt to explain Darwin’s bewildered (although somewhat inaccurate) statement that after coming to London Grant “did nothing more in science” (11).

Grant, the first Professor of Zoology at the new utilitarian London University in 1827, was possibly the only teacher in Britain sympathetic to transformism in the three decades before 1859. An internalist historian of biology might argue that this heresy alone was enough to damn him in the turbulent thirties, and that little more is needed to explain his demise. After all, Lamarckism came complete with obnoxious auxiliary hypotheses: spontaneous generation, monads, inexorable progression, lack of design – the cumbersome “machinery” so anathema to Established Church dons and divines like Sedgwick, Whewell, and Buckland (12). And it conjured up that most haunting of Victorian fears, the brutalization of man (13). Fall from grace he did. Having risen smartly in the early thirties, patronized by radicals, notably the fiery agitator Thomas Wakley, and having penetrated the sanctum sanctorum, the Council chamber of the Geological Society, by the 1840s he had stopped publishing and was reduced to a “slum” existence (14). By 1850 a public subscription had to be got up to save him from “absolute penury” (15). In calling him that “shadow of a reputation”, Forbes was alluding to Grant’s failure to live up to his early promise – to become what *The Lancet* had prophesied, the “English Cuvier” (16).

These statements suggest political questions. Note that it was the *Oxbridge* dons who stood in outright opposition at the time of the divisive Reform Bill, and Wakley’s radical *Lancet*, with its working-class ideology, which underwrote

Grant's secular biology and social rise. This social divisiveness will figure prominently in the present study, and I shall suggest that Grant's transmutationist biology can be treated as an expression of deeper reformist concerns. (Although such a statement tends to reduce Grant's science to secondary status: perhaps it would be fairer to say that his morphological construction and political action were both functions of an underlying reformist ideology.) His subsequent decline was indeed complex, involving political, economic, and personal factors. His socially-levelling views had repercussions at the learned societies dominated by established coteries. His denunciation of privilege at the College of Surgeons and support for the "*canaille*" politically distanced him from a college employee like Owen. He became caught up in professional rivalries and suffered financial hardship, partly as a result of his loss of patronage following his defeats at the hands of Tory factions at the societies. And possibly he alienated many by his breach of contemporary mores. To this has to be added the taint of materialism at a time when anti-Providentialism was an expression of working-class radicalism. With materialists slated by pious working-class evangelicals like Hugh Miller as invariably "bad men" (17), one begins to realize that Grant's demise was a demonstrably social affair. His outspoken views were destructive to gentlemanly standards. He violated contemporary ideas about what made a good savant and citizen.

Some Historiographical Problems

Of the innumerable problems involved in tackling so controversial a figure, the most serious are historiographical. Because Grant befriended teenage Darwin at Edinburgh, he has invariably been relegated to a footnote in Darwin studies.

Scholars have been encouraged to keep him in this context by the fact that the most accessible information on the man is contained in Darwin's and Huxley's works (18).

This is doubly unfortunate because leading Darwinians came to disown Grant; only the materialist physicist John Tyndall had a kind word to say in his notorious Belfast Address, delivered within a few days of Grant's death in 1874 (19). Nor was the fact that Darwin himself jotted "Nothing" on each of his parts of Grant's *Outlines of Comparative Anatomy* (1835-41) guaranteed to enhance Grant's reputation (20).

Evidently Darwin while compiling the "Transmutation Notebooks" assumed that there was little of use in Grant's Parisian comparative anatomy, for he even scribbled "Nothing" on the last – and still *unopened* – part of the *Outlines*. To be fair, Grant did not discourage his name being linked to Darwin's after 1859; he actually basked in the reflected glory, which hinders any attempt to prise him from a Darwinian context. No sooner had the *Origin* appeared than he published a little book, *Tabular View of the Primary Divisions of the Animal Kingdom* (1861), dedicated it to Darwin, and in an open letter reminded his former pupil of

their time together in Edinburgh and their subsequent labours “in the same rich field of philosophic inquiry” (21).

As a result Grant has never been subjected to a contextual study, or considered purely on his own terms. But then the canons of an older positivist historiography practically precluded this. *The Lancet* reviewing the *Tabular View* realized that there could be “no doubt” on which side of the “Darwinian controversy” Grant stood (22). Yet it ignored the astonishing divergence between Darwin’s and Grant’s mature views, which are obvious even at a cursory glance. Grant’s “Palaeozoology” lectures in the 1850s underpinned an archaic serial ascent (Ch. 8 and Appendix); and his abiogenetic model by 1861 supported a profoundly unDarwinian superstructure. He never relinquished his belief in equivocal generation (23), and his ‘genealogical’ image was one of *multiple* trees, each presumably rooted in independent monadial stock (24). Yet obituarists still talked of his having been “far ahead” of his time (25). This implied, in the words of the mercantilist proprietor John Evans, that Grant had anticipated to some extent “the doctrine of the origin of animal forms by descent with modification” (26). But Grant never used such overtly Darwinian terminology, and it belies the true nature of his concept of Geoffroyan “metamorphosis” or fossil “generation” (27). Talk of his being “far ahead” tells us more about Victorian historiography than Grant’s own beliefs – the sort of historiography which scanned the past for precursors and

then portrayed them as fully and finally eclipsed by Darwin's back-projecting shadow (28). Ultimately a deconsecrated Sedgwickian palaeontology, Paleyite theology, and Lyellian methodology proved more congruent with Darwin's social needs and scientific position – as a Cambridge-educated careerist of ample means patronized by the elite of the Geological Society. Grant's hurried professionalism, his guinea-grabbing production line at London University churning out students to the detriment of research, his anti-elitist alignments in these radical years, all made his rationalist faith in fossil and social progress look politically suspect and scientifically irrelevant to the Oxbridge Anglicans dominating the Somerset House society. Any points of contact between Darwinism and Grantism, like a common understanding of 'generation', were lost as Darwin himself assumed the mantle of responsibility at the society. From here he watched as his new Peelite friends Buckland and Owen publicly bullied Grant for his progressive affiliations (Ch.7).

Historians now find it more profitable to take a contextual approach and investigate the cultural and scientific factors which might have sustained earlier developmental theories. Jon Hodge points out in his study of *Vestiges of the Natural History of Creation* (1844) that when we cease trying to read the *Origin* back into earlier works, they sometimes show up in a surprising light (29). Whether one tackles the subject from the standpoint of individual psychology or cognitive sociology (30), it makes little sense

to imagine Darwin's culturally-scattered 'precursors', all seeking an answer to a common problem. In the same way, decontextualization reduces the explanatory power. It would be impossible, for example, to interpret Grant's radical biology from an *1850s* social perspective. This was a time of vastly altered social and economic conditions – of "equipoise", stability and amelioration of class antagonism, of dissident literature, and bourgeois ascent in London science. Grant's Lamarckism is firmly anchored in a turbulent thirties' context – a time of Tory fears, working-class demands, Chartist, mob violence, and a vociferous radical party in parliament. Given this new contextual emphasis, the time is ripe to investigate the generation of 'Grantism' in reformist Edinburgh circles and to understand its endorsement by doctrinaire radicals in London. Only having done this will we appreciate the social functioning of Grant's imported Parisian science.

So Grant's 'predicament' reflected considerably more than his espousal of scientific heresies. In the 1840s it has to be seen against a background of declining radicalism. It reflects his shrinking power base in establishment institutions like the new Zoological Society, where the Tory 'Junto' representing the gentrified backers contrived to oust him from the Council; his loss of resources and funding was then exacerbated by the laissez faire financial arrangements at the joint stock university. And his refusal to condone the

monopoly of the Royal College of Physicians by sitting a licencing examination left him unable to practice in the city. All of these actions were inextricably tied to the same radical ideology which sanctioned a socially-subversive anti-Providential Lamarckism. This institutional discouragement and loss of patronage resulted in his dramatic publishing decline and finally to his misanthropic restructuring of palaeozoological classification in the 1850s (Ch. 8 and Appendix). So what we are really investigating is the way science was integrated into strategies devised to achieve wider social and political goals.

The reciprocal relationship between the impact of Grant's non-Paleyite morphology on pupils and the failure of his name to live on among 'disciples' provides a historiographical paradox. His obscurity can best be highlighted by putting him in a comparative light. Why was he never considered as influential as his colleague at University College, William Sharpey, a teacher who published surprisingly little but was visited with the inevitable title, "Father of Modern Physiology"? (31). Part of the answer, as suggested by Gerald Geison, is that Sharpey '*grandfathered*' a successful research school, via his (and Grant's) pupil Michael Foster. Grant can hardly be said to have founded a 'school' in this sense, or at least not a successful one. The abiogenetic aspect of his development doctrine was championed by his 1859 Gold Medalist Henry Charlton Bastian (1837-1915), who as Professor of Pathology at University College was to praise Grant and

pursue spontaneous generation to the end of his life (32) But he met considerable resistance from the Darwinians, Huxley, and Tyndall in particular (33), causing Grant to commiserate with him in 1872, “Until we can actually see the atoms ... select their partners and waltze off in a quaternary danse [sic] of life, there will always be an excuse for cavilling on this matter” (34). Although sympathy for abiogenetic development was shown by other materialists among Grant’s medalists, for example Alexander Herzen, son of the exiled revolutionary and by the 1870s professor of physiology in Florence (35), in face of stiff Darwinian opposition and rival bourgeois support for Haeckelian monophyly (36), it was to remain a minority evolutionary doctrine destined for extinction.

The problem of Grant’s influence as a *Geoffroyan* in the 1830s is quite different. One of the main *internalist* tenets of this thesis is that his non-Paleyan structuralism was crucial to the shift away from Cuvierian teleology initiated at this time. Peter Bowler and Dov Ospovat (37) have acknowledged this advance from Paleyite design to a theology of archetypalism or ‘unity of plan’ restricted to each *embranchement* in the late 1830s. I will suggest that Grant helped instigate this move earlier in the decade, and thus that he can be assigned an historical importance. By way of corollary, one of the main *contextual* tenets of the thesis is that contemporary social shifts can be directly

correlated with this migration towards Geoffroyan morphology. Further, the political context itself may even help to explain why Grant is no longer credited with influence in this area. Thus his *extreme* Geoffroyism, with its extended ‘unity’ supporting taxonomic continuity and transformism, waxed at the time of heightened radical demands in the early 1830s, after which his Unitarian, middle-class pupil W. B. Carpenter adapted it for more bourgeois tastes by fashioning from it a more moderate archetypalism (i.e. he limited the taxonomic extent of the unity), while in Owen’s hands it became a model for more cautious reform: a designful anti-transformist structural system perfected at the time of Peel’s ministry (the mid-1840s). Ospovat’s and Bowler’s internalist analyses ignored both the changing shape of structural morphology and its shifting political context. They also ignored Grant; and the reason might possibly have to do with the fact that this shift in political allegiance would have actually made it undesirable for more conservative morphologists to acknowledge the doctrine’s *radical* roots.

Another reason for Grant’s latter-day obscurity is that he was dismissed by leading Darwinians. One might have imagined that a self-styled “plebeian” like Huxley would have applauded the only materialist “evolutionist” teaching in pre-Darwinian London. After all, Huxley had by the 1860s abandoned Carlylean romanticism and taken a more aggressively militaristic stand, signalled by his attacks on “Parsonism” (38). He knew Grant, who had helped him prepare for his trip

to New Guinea in 1846 (39) and continued to loan him specimens. As late as 1873, a year before his death, Grant was attending “The Sunday Lecture Society” in St. George’s Hotel with the new *enfants terribles* Clifford, Huxley, “and other good men & true” (40). But Huxley evidently entertained little respect for the old materialist. In 1850 he considered that Grant had missed his vocation, and he later wrote bitingly that Grant had done nothing to advance the evolutionary cause (41). There are a number of plausible reasons: Grant was one of the cynical materialists “who delight in degrading man” despised by Huxley, and Grant’s radical politics formulated in the turbulent thirties when Chartists took to the streets would have been too extreme for Huxley’s liberal tastes. And despite Grant’s “evolution”, his cosmic pessimism, anachronistic serial succession, nebular cosmology, and spontaneous generation, ensured that the sexagenarian was excluded from the new Darwinian pantheon. Even aspects of Grant’s personal life might have seemed open to question (42) – as Forbes’ remark to Huxley in 1852 indicates, observers were daily expecting some new “eccentricity” to manifest.

A more mundane problem contributing to Grant’s subsequent obscurity concerns the loss of his personal letters. According to his *Lancet* biography of 1850, he burnt manuscripts of published works but preserved extensive journals and volumes of letters (43). What happened to these

is not known. He never married, and died leaving no close relatives. Sharpey persuaded Grant on his deathbed to bequeath his books and instruments to University College, and urged that his money be used “to establish a fund for maintaining and expanding the Zoological & Zootomical department of the Library” (44). Sharpey makes no mention of the letters, nor do they figure in Grant’s will. Since he was in the habit of burning manuscripts, he possibly destroyed them. Anyway, lack of letters, the rarity of his books, and denigration by early Darwinians all help explain the scant attention that has been paid to this London Lamarckian.

Explaining Grant’s fall into obscurity, though, is not the main aim of this thesis. Rather I intend to investigate the more significant problem of the political *meaning* of his morphological science *in the radical thirties*, and interpret the social and scientific reaction of a Peelite protagonist like Owen. The thesis is divided into parts, the modified substances of which have appeared (or will appear) in four papers. Ch.2 deals with the generation of Grant’s reformist biology in late Enlightenment Edinburgh and Ch. 3 with his transfer to the Benthamite London University (LU), and the problems arising over finances, lecture arrangements, and his loss of foothold at the societies, especially the Zoological Society (ZS). There follows a discussion of the structure of his science, its partisan political reception, and its influence in directing morphology away from Cuvierian functionalism (Ch.4). The subject matter of these chapters is

to appear as: "Robert E. Grant (1793-1874): The Social Predicament of a Pre-Darwinian Transmutationist", *Journal of the History of Biology*, 17 (1984), in press.

Ch.5 deals with Grant's radicalism, democratic demands, and attacks on the chartered corporations, and with Owen's Coleridgean, patriotic conditioning at the College of Surgeons (RCS), itself under siege from democratic Wakleyan forces.

Ch.6 deals with Owen's *scientific* response: his restructuring of monotreme, ape, and saurian morphology to discredit a secular, socially-subversive Lamarckism. Ch.7 tackles the positive heuristic value of his anti-transmutatory ideology, in leading him to a study of von Baerian embryology. As a result he was able to construct a more sophisticated morphology and palaeontology. Finally this chapter explores Owen's and Buckland's machinations at the Geological Society (GS) to out-manoeuvre Grant, Owen's Peelite connections, and the massive patronage he received from the Oxbridge managers of the learned societies. A modified form of Chs.5-7 will appear as: "Richard Owen's Reaction to Transmutation in the 1830s", *British Journal for the History of Science* (1984-5), in press. A study of the mechanics of Owen's reconstruction of saurians for ideological ends is to be found in an appended paper, "Designing the Dinosaur: Richard Owen's Response to Robert Edmond Grant", *Isis*, 70 (1979), 224-234.

Finally, Ch.8 deals with Grant's "Palaeozoology" lectures of the 1850s and the growing anachronism of his serial development. Falling outside my timespan, these later lectures are only mentioned fleetingly here, to illustrate the classificatory consequence of his disillusionment. A longer paper is bound in separately as an appendix: "Robert E. Grant's Later Views on Organic Development: The Swiney, Lectures on 'Palaeozoology', 1853-7", *Archives of Natural History*, (1984), in press.

This thesis, then, attempts to assess the political relevance of contemporary biology, and to interpret changing morphological structure from the standpoint of both available resources and the work it was expected to do within rival institutional and class contexts. It thus seeks to underscore internalist studies of biology with a more satisfying social explanation: in short, to argue that radicalism and Peelite compromise in the Reform years are as inextricable to explanations of the esoteric structure of zoological knowledge as to any other part of the social superstructure.

Notes and References

Abbreviations used throughout references:

BL	British Library
BM(NH)	British Museum (Natural History)
RCS	Royal College of Surgeons of England
UCL	University College London
EPJ	Edinburgh Philosophical Journal
ENPJ	Edinburgh New Philosophical Journal
TZS	Transactions of the Zoological Society of London

Grant, “Lectures”, refers to his sixty “Lectures on Comparative Anatomy and Animal Physiology” in *The Lancet*, vols. 1 and 2 (1833-4), *passim*.

BS “Biographical Sketch of Robert Edmond Grant”, *The Lancet*, 2 (1850), 686-95.

1. R. Owen, “At the Anniversary Dinner (Hunterian)”, n.d., File Misc./Hunt. D(l), MS RCS.
2. E.g., P. J. Bowler, *Fossils and Evolution: Paleontology and the Idea of Progressive Evolution in the Nineteenth Century* (New York, Science History Publications, 1976); A. Desmond, *Archetypes and Ancestors: Palaeontology in Victorian London 1850-1875* (London, Blond & Briggs, 1982), Ch. 4.
3. In addition to the above Dov Ospovat’s work is most important, esp. his “The Influence of Karl Ernst von Baer’s Embryology, 1828-1859: A Reappraisal in Light of Richard Owen’s and William B. Carpenter’s ‘Palaeontological Application of von Baer’s Law’”, *J. Hist. Biol.*, 9 (1976), 1-28; idem., “Perfect Adaptation and Teleological Explanation: Approaches to the Problem of the History of Life in the Mid-Nineteenth Century”, *Stud. Hist. Biol.*, 2 (1978), 33-56; idem., *The Development of Darwin’s Theory: Natural History, Natural Theology, and Natural Selection, 1838-1859* (Cambridge University Press, 1981).
4. Desmond, op. cit. (2).
5. S. Shapin, “Phrenological Knowledge and the Social Structure of Early Nineteenth-Century Edinburgh”, *Annals of Science*, 32 (1975), 219-43; idem., “The Politics of Observation: Cerebral Anatomy and Social Interests in the Edinburgh Phrenology Disputes”, in R. Wallis (ed.), *On the Margins of Science: The Social Construction of Rejected Knowledge*, Sociological Review Monograph 27 (1979), 139-178.

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II (London, Longman, 1879), 6th. ed., 137-203 (174).

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26. J. Evans, "Anniversary Address", *Quart. J. Geol. Soc.*, 31 (1875), 11.
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34. R. E. Grant to H. C. Bastian, 26 June 1872, Wellcome Institute for the History of Medicine Library.
35. A. Herzen to H. C. Bastian, 15 December 1877, Wellcome Institute for the History of Medicine Library. Herzen was Grant's 1858 Gold Medalist.
36. Desmond, op. cit. (2) for Haeckel's strength in Britain; also of interest is J. Farley, "The Initial Reactions of French Biologists to Darwin's Origin of Species", *J. Hist. Biol.*, 7 (1974), 275-300 (290-4).
37. P. J. Bowler, "Darwinism and the Argument from Design: Suggestions for a Reevaluation", *J. Hist. Biol.*, 10 (1977), 29-43. Ospovat, "Perfect Adaptation", op. cit. (3).
38. Desmond, op. cit. (2), 81-2 on the strategic value of Huxley's gladiatorial posture. Also on Huxley's militarism: J. R. Moore, *The Post-Darwinian Controversies: A Study of the Protestant Struggle to Come to Terms with Darwin in Great Britain and America 1870-1900* (Cambridge University Press, 1979); and J. G. Paradis, *T. H. Huxley: Man's Place in Nature* (Lincoln, University of Nebraska Press, 1978) on his migration from Carlylean romanticism.
39. T. Wharton Jones to R. E. Grant, 8 April 1846, Huxley Papers, Vol. 19, f. 86, Imperial College. L. Huxley, op. cit. (18), i, 25 mentions this introduction. Huxley continued sending Grant offprints, e.g., R. E. Grant to T. H. Huxley, 18 December 1851, Huxley Papers, Vol. 17, f. 106a. And it was the crocodiles from Grant's museum that Huxley used in his researches: M. Foster and E. R. Lankester, *The Scientific Memoirs of Thomas Henry Huxley* (London, Macmillan, 1898-1902). 287-8, 309.
40. R. E. Grant to H. C. Bastian, 10 February 1873, Wellcome Institute for the History of Medicine Library.
41. L. Huxley, op. cit. (18), i, 94; Huxley, "Reception of the 'Origin'", op. cit. (18), 188. Huxley was a past master at dispatching an Owen or a Grant, on which see M. Ruse, *The Darwinian Revolution: Science Red in Tooth and Claw* (University of Chicago Press, 1979), 741-2.
42. It has been suggested to me (R. B. Freeman, pers. comm.)

that Grant was a homosexual; while it is impossible to substantiate this, if true it would have added nothing to his worth in the eyes of respectable family men like Owen and Huxley. Nor would it have restored Owen's faith in the moral progress of man promised by social reformers in the 1830s.

43. BS, 686-95 (692). This is the main biographical source for Grant, and comes complete with Wakley's radical embellishments.
44. W. Sharpey to J. Robson, 22 August 1874, MS College Collection, UCL.

Chapter 2

Grant and the Social Nexus of Materialist- Transmutationist Thought in Edinburgh in 1826

When social scientists study scientific knowledge the focus of attention is the knowledge associated with a specific social context. The answer must expose every sociologically interesting factor which has a bearing on this question. The assumption is that such factors are always involved in the network of causes which maintains the credibility of a body of knowledge.

Barry Barnes, *T. S. Kuhn and Social Science*
(1982) (1).

In this chapter I want to mix the biographical and social approaches; biographical because we need to locate and identify Grant's specific scientific views, and social because I believe that it is only possible to *explain* how a materialist theory of transformism was sustained in Edinburgh by socially embedding it. In other words, we shall investigate what Barnes calls the "network of causes" which maintained the credibility of Grant's brand of deistic Lamarckism within a specific cultural context. Although I will not take a rigorously prosopographical approach here (2), I will show the explanatory value of processing the views of contemporaries who experienced similar cultural 'moulding'. These include the students who, with Grant, passed through the Edinburgh High School in the 1800s and 1810s, and matriculated at Edinburgh University during the second and

third decades of the nineteenth century. We need to examine a complex of educational social, political, and scientific influences to understand why the medical school should have produced such a stream of Whig-reformist graduates – often of outstanding ability and materialist views – a group which became enamoured of the new Parisian comparative anatomy of Cuvier and transcendental morphology of Geoffroy St. Hilaire, and even of Lamarckian transformism. The city of Edinburgh was to supply some of the most determinedly materialistic physicians of the early to mid-nineteenth century, men like the Geoffroyan cleric-baiter Robert Knox, the materialist phrenologist and moral mad-doctor W. A. F. Browne, radical G.P. union organizer George Webster, and the Lamarckian deist Grant. Many Edinburgh-trained political and scientific activists migrated to London in the 1820s, where they took up the cause of medical reform, some becoming associated with the new Gower Street university.

From the outset I shall therefore take issue with Loren Eiseley, who decontextualized evolutionary history and consequently found Grant “something of an anomaly” in the Scotland of his day (3). Set against a *standardized* representation of the Presbyterian Kirk, Grant might appear ‘anomalous’ to us. But it is unproductive to dissociate his revisionist biology from its radical base, since it is through this political context that he can be related to medical men active in other areas. In truth any glib homogenizing does an injustice to the fine texture of Scot-

tish cultural history and generalizing Kirk attitudes produces only a caricature. It belies the strongly demarcated cultural loci of the numerous social and political groups in Grant's Scotland – a Scotland suffering tensions (4) in the aftermath of the Revolutionary Terror and Napoleonic Wars, and experiencing political extremes, from Henry Dundas's diehard Church-and-King Toryism at the turn of the century to Robert Owen's utopian New Lanark 'socialism' in the 1820s. Jack Morrell (5) has studied the effects on institutionalized science of respective Tory and Whig allegiances, as well as the friction between the Edinburgh Town Council (under whose patronage most of the chairs fell), the Senatus, and the Moderates and evangelicals of the Church of Scotland. It is also clear from Shapin's studies on the Edinburgh phrenologists and those of Roger Cooter on the mad-doctors that some reformist groups were inclined, for various disestablishmentarian, Nonconformist, or other anti-Kirk reasons, to favour a form of secular materialism. The danger for us, in discussing the philosophic materialists *inside* the University, is to think too strongly in terms of sub-cultures. It is wrong to imagine them segregated into something like the "cultic milieux" (6) of the sociologists of religion. On the contrary, they apparently mixed freely with conservative teachers of Presbyterian piety. Nonetheless the materialist MDs and their students *were* ultimately opposed on ideological grounds to the elite teachers in the University; and therefore we *should* be alert to the erection of 'cultic'

barriers to protect those holding heretical ideas from conservative attack.

It could be argued against Eiseley, that when we do widen our horizon and take into account Grant's colleagues, we find that he was actually outflanked, e.g. in his materialistic philosophy by W. A. F. Browne, while others like Robert Knox pursued Geoffroyan anatomy no less vigorously. It can pay to take a more open-minded social approach. We need to examine the manuscript evidence relating to student activity, particularly at the Plinian Society, where heterodox views were openly expressed. This should help us to understand the sort of contemporary reformist ideologies which could have helped to sustain Grant's deistic biology. Certainly there is a pressing need to account for the prevalence of materialistically-inclined graduates at this time, and to explain why even those who entered the University as ardent evangelicals, like John Coldstream, should have subsequently been racked with materialist doubts and have suffered spiritual crises.

So the first task, after discussing Grant's Whig education at the High School, is to investigate the reformist milieu in which he worked, and to establish that, far from being anomalous, he shared the materialist presuppositions of many medical men, even while his Presbyterian mentors, John Gordon and Dr. John Barclay, and the Minister of Flisk, John Fleming, remained staunchly anti-mechanistic.

Robert Grant and Robert Knox

His reasonings sounded of transcendentalism, whilst his talk breathed a doubting theism terribly incongruous with human creeds and low idealistic faiths.

Henry Lonsdale, *A Sketch of the Life and Writings of Robert Knox* (7).

To strengthen my thesis, I shall run Grant's biography in parallel with that of a more celebrated anatomist, Robert Knox (1793-1862). This is for a number of reasons. Knox was Barclay's successor and the best-attended teacher of anatomy in Edinburgh in the later 1820s. As such he was better known than Grant. And of course he gained notoriety as a result of the Burke and Hare scandal of 1828, which has made him an attractive subject to biographers. Hence we have Lonsdale's 420-page "Sketch" (1870) and more recently Isobel Rae's *Knox the Anatomist* (1964). This contrasts sharply with the paucity of biographical material on Grant. Another reason for treating them together is that they were exact contemporaries, had an identical schooling, and ended up holding closely similar deistic and radical views. Later I shall introduce a number of other actors to support my contention that a radical materialism was more widespread in Edinburgh, even among the University students and graduates, than is often accepted. And since Darwin was placed under Grant's wing in this period, and heard Grant extol the virtues of *Flustra* and Lamarck alike, the present study should throw additional

light on the kind of extra-mural free-thinking environment the freshman found at Edinburgh in 1825-7.

Grant was born in Edinburgh in 1793, the seventh son of a professionally-successful writer to the signet (8). One of fourteen children, he outlived all ten brothers (most of whom entered the armed services or the East India Company, and died abroad (19)). The family engaged a private tutor for twenty years. Grant's religious background was said to have been "free from bigotry and intolerance" (10); and although his deism later gave way to secularism and probably atheism (11), he never lost a historico-linguistic fascination for the Bible, and like later secularists (e.g. T. H. Huxley) retained an intimate acquaintance with chapter and verse. Knox was likewise born in Edinburgh in 1793 (12) and tutored at home. He was the fifth of eight children. His father was a teacher of natural philosophy and later mathematics master at George Heriot School. The elder Knox was a Freemason and at the outbreak of the French Revolution a member of the democratic "friends of the People" sympathetic to Jacobin ideals – although he withdrew from the society before the movement was crushed. The Freemason connection is telling. The younger Knox was himself enrolled in the Paris Lodge of Freemasons in 1822; because of Continental Freemasonry's supposed religious and political subversiveness, Rae concludes that Knox must already have been deeply anti-clerical. Politically he remained uncompromising and

iconoclastic (and an ardent admirer of Napoleon, suggesting that he was a conservative radical (13)). This implies a strong anti-Tory element in his upbringing. The crushing of the “Friends of the People” and sentencing of its leader Thomas Muir to fourteen years’ transportation to Botany Bay in 1792 and the hysterical attack on Freemasons as subversives by Tory John Robison in *Proofs of a Conspiracy* (1797), meant that as a Freemason family the Knoxes were under grave suspicion – their views being denounced by Scottish Tories and the Established Church as democratic and traitorous (14).

Grant and Knox in their teens both attended Edinburgh High School, Grant from 1803-8, and Knox from 1805-10 when he graduated as gold medallist or Dux (15). T. C. Smout has already commented on the peculiarly Scottish mix achieved in the school, where a future Marquis might sit alongside a shopkeeper’s son. In fact the children of professional families predominated, with a decline in those of the landed classes in the early nineteenth century as the merchants began their rise in society. A considerable number of the future medical men discussed in this chapter passed through the school, including Browne, Coldstream, and social reformers Andrew and George Combe. At the school Grant and Knox received a classical education with Grant himself excelling in Greek and geometry. Greek had been taught for forty years by the rector Dr Alexander Adam (1741-1809), who was known to have expressed liberal political sentiments in class. He ardently supported constitutional freedom (16), and

believed that the American colonies would never have been lost had British politics taken a more enlightened turn. He had taught a succession of students who were later to make their mark as reforming Whigs. Among the most distinguished the school could boast the future Whig Lord Chancellor Henry Brougham (1778-1868), Henry Cockburn (1779-1854), who spurned the high Toryism of his father and Dundas-in-laws to become a prominent Whig advocate and architect of Scottish reform, and the brothers Horner, Francis (1778-1817), founder of the *Edinburgh Review* and Whig MP, and Leonard (1785-1864), geologist, educator, and first Warden of London University. So future reformist politicians and radical scientists (those responsible for founding the mercantilist London University and teaching in it) were receiving a liberal, democratic High School education in spite of the Tory reaction to the Revolution. To begin to understand Grant the Francophile transformist and medical reformer one has to start with this Edinburgh Whig-reformist environment. To what extent medical students at the university who *lacked* this Whig schooling differed from their peers can only be conjectured. Take the case of the future Bristol physician John Addington Symonds (1807-1871). He came from a respectable Shropshire family, and was educated at Magdalen College School, Oxford, where he was taught anatomy and chemistry by the Oxford dons (John Kidd and Charles Daubeny respectively). Although Symonds studied at Edinburgh University during the years when Grant and Knox were teaching (1825-8; he graduated MD in 1828), and

although familiar with members of the Plinian Society, he was never as materialistic as Browne or W. R. Greg, or as deistic as Grant and Knox – even though he did share a number of Edinburgh traits, in particular love of transcendental anatomy, and later openness to higher Biblical criticism and providential evolution (17).

Grant entered Edinburgh University in 1808, two years before Knox, and began by taking the literary classes, whilst Knox proceeded directly to medical studies. As a result they graduated MD on the same day, 14 June 1814. One of the few differences in their medical education was that Knox (like so many students bored with Monro *tertius*'s dry lectures) forsook the official anatomy classes in 1813 for the more stimulating extra-mural lectures of John Barclay (1758-1826), whereas Grant attended John Gordon's rival school, and did not enter Barclay's classes until the winter of 1820 (18). Gordon was clearly influential in directing Grant to comparative anatomy, but as we will now see Grant's earliest preserved manuscripts reveal him already distancing himself from his mentors on ideological grounds.

Grant and John Gordon – Disagreements over Gall and the Question of Materialism

While the subject of this first chapter is Grant's importation of Lamarckism it is first necessary to tackle his anti-orthodox leanings in order to understand why Parisian

materialist doctrines might have looked so attractive. In this section I examine his early manuscripts to locate him on the fringes of the Edinburgh phrenology debate – a debate, as Shapin has shown, with class overtones, and one inextricably concerned with the validity and political functioning of materialist scientific doctrines. Since it is likely that Grant’s doctrines were *sustained* within the materialist confines of the Plinian Society, which also nurtured aspects of phrenological reformism, this extra-mural Edinburgh context is directly relevant to any explanation of his adoption of French transformism.

Grant was interested in comparative anatomy by his early teachers Andrew Fyfe (1754-1824) and John Gordon (1786-1818), both of whom he was attending by his second year (1810). He sat several of Gordon’s courses in anatomy and physiology, while Fyfe he called his “esteemed and intimate friend” (19). Fyfe, for forty years “the plodding practical demonstrator” for Monro *secundus* and *tertius* (20), was by every account an appalling teacher, but a singularly successful compiler of student texts. A self-effacing, downtrodden man, he admitted that his *Outlines of Comparative Anatomy* (1813) – confusingly the fourth volume of the fifth edition of his popular *Compendium of Anatomy, Human and Comparative*, issued separately – was merely a compilation from Blumenbach’s and Cuvier’s works (21). Yet Grant, champion of the underdog and later voice of the common man, remained supportive and in a lecture to the Royal Medical Society in 1814 argued that the *Outlines* dis-

plays “sufficient originality to entitle him to be considered as one of the chief benefactors of this science” (22).

The preserved draft of this lecture, “An Essay on the Comparative Anatomy of the Brain”, shows Grant already uneasy with the anti-mechanist orthodoxy of the established anatomists. It was the last of four memoirs (23) delivered to the Medical Society, and read in October 1814 – the year in which he was elected President of the Society. In it he was characteristically sarcastic about the scientific predilections of his mentor John Gordon, to whose “eloquent lectures” he had admitted owing his own interest in anatomical pursuits (24). Although Gordon’s career was short (he died from an infection contracted at the Royal Infirmary when only 32) he was considered the most promising teacher and a rival to Barclay himself. There was even friendly competition between the two, who lectured next door to one another in Surgeons’ Square, although Gordon’s class of less than 100 never matched Barclay’s in size (25). By the time Grant delivered his final memoir to the Medical Society, Gordon’s interest was focussed on metaphysical aspects of physiological theory – specifically theories of life, the seat of sensation, and the independence of mental phenomena “from mere Matter” (26). In 1814 and 1815 he wrote three (27) articles for the *Edinburgh Review* which can be taken together as an integrated attack on the craniologists’ doctrine that the brain is the organ of mind and that thoughts have a physical substrate. (Only the

third review, on “The doctrines of Gall and Spurzheim”, explicitly tackles the issue, but the earlier two formed an essential prologue.) Gordon was no crude vitalist; his first review demolished Abernethy’s bombastic “collection of bad arguments” in defence of some quasi-electrical vital fluid, which Gordon derided as one of the “most untenable speculations in physiology” (28). He considered that living bodies were differentiated by their chemical activity and utilization of foreign matter: life, in short, was a process not a “Principle”. And yet it could not be characterized merely by sensation or excitation or thought. Indeed he disputed the common wisdom “that the change in [the nervous] system preceding sensation, is one which is propagated from the nerves to the brain”, claiming that

this opinion seems to us to rest on very unsatisfactory grounds; and we shall take an opportunity of stating our doubts more particularly on this point, in another Number, in the course of our remarks on those huge quartos of absurd theory, which Gall and Spurzheim continue to pour forth (29).

In fact he did not proceed directly to challenge Gall and Spurzheim. His next review rather capitalized on Everard Home’s observations on brain lesions. Gordon turned these to advantage by instancing cases where destruction of lobes and sometimes “the *whole* brain” had occurred without “loss of sensibility” (30) (the latter being the case of children born and apparently remaining alert for some days with only remnants of a medulla). So in the *Edinburgh* number preceding

his assault on Gall, he was able to conclude that the brain was inessential to the changes preceding sensation, and this conclusion provided his anti-materialist baseline from which to attack Gall.

Gordon reopened the public debate on craniology in Scotland with his *Edinburgh* critique in 1815. The social contours of the phrenological debate are fairly well known. G. N. Cantor has examined the opposition to craniology put up by the moral philosophers, in particular Dugald Stewart's successor Thomas Brown, and Sir William Hamilton. Cantor concentrates on the professional threat: the phrenologists invaded territory claimed by the Edinburgh anatomists and moral philosophers by demanding that dissection of the brain could no more explain mental functioning than introspection could form the basis of a successful philosophy; only an empirical study of the psychological faculties through phrenological analysis was adequate to the task (31). That there might have been more to the debate is suggested by the attitude of the same philosophers towards equally materialist, but professionally non-threatening subjects such as Enlightenment evolution. Brown was himself a graduate in medicine, and had already subjected Erasmus Darwin's mechanistic physiology and philosophy of mind in *Zoonomia* to a penetrating critique (32). In 1815 the sort of anti-materialist contumely once heaped on Darwin was reserved for Gall. Cantor concludes that the moralists' ideology was shared by the students (33). But such a statement requires

careful qualification. It is true of leading figures like Gordon and Barclay. However when one begins to look from below, at the younger Grants and Knoxes in the 1810s, and the more stridently materialistic students in the 1820s (some like Browne committed phrenologists), the picture becomes more complex. A newly-graduated MD like Grant suffered no such mechanistic qualms and was receptive to both Darwin's laws of organic life (34) and Gall's revisionist anatomy.

Gordon was Dugald Stewart's student and the most vociferous anti-craniologist of the period. It is well known that he considered the "whole doctrine taught by these two modern peripatetics [Gall and Spurzheim], anatomical, physiological, and physiognomical, as a piece of *thorough quackery* from beginning to end" (35). He was worried into print by the growing number of converts – he despaired of those "dupes of empirics" who swallowed such nonsense, and described Gall and Spurzheim in quasi-religious terms as "itinerant philosophers" seeking proselytes wherever they went. Gordon claimed that they could never deceive professional anatomists (36), and Grant's pronouncements at the Medical Society in 1814 indicate that aspects of Gall's cerebral anatomy *were* acceptable to younger medical men. It is even possible that transformist and/or social environmentalist considerations actually entered into the phrenological debate. Gordon agreed with Gall that none of the faculties, "The propensities and sentiments" (37), are

caused by external influences, nor were human faculties the product of social conditioning. The faculties are innate and depend on “*internal causes*”, a fact, Gordon intimated, that ensured a certain creative stasis (i.e. it had anti-transformist connotations), since no animal could be induced through environmental conditioning to alter its specific characters (38). In the same way, man’s faculties “are *given by creation*”. Gordon’s aim was to point up “the trash, the despicable trumpery” of organology, but his strictures applied alike to Lamarckian explanations in biology and Owenian socialist utopias. Primarily, Gordon was concerned to negate materialistic philosophy: the belief that “all the *manifestations* of the mind – feelings and intellectual faculties – depend on organization” (39), and it was this faith that he had already begun undermining in his previous *Edinburgh* reviews.

The esoteric anatomical aspects of the Edinburgh phrenology debate are also becoming better known. As part of his programme to determine the pre-structuring of scientific knowledge by social interests, Shapin has analysed the conflicting claims arising from Gordon’s and Gall’s studies of the gross morphology and tissue structure of the brain. There remains some dispute over the extent to which social interests operated *in 1815*, that is, before George Combe’s conversion by Spurzheim (who visited Edinburgh in 1816-17, in response to Gordon’s challenge). Cantor sees few *explicit* references to social reformism in phrenological writings

before the later 1820s. And it is true that Spurzheim in the *Physiognomical System of Drs Gall and Spurzheim* (1815) does not articulate a meliorist social programme, mentioning only in passing that “the means of perfecting mankind” rests in physiognomic practice (40). Shapin however draws on the philosophical *presuppositions* of craniologists (rather than statements of reformist intent) to suggest that the work phrenology was expected to do was fully in accordance with Spurzheim’s or the Edinburgh phrenologists’ ideological interests. He claims in the “Politics of Observation” that

At the time of Spurzheim’s first English publication in 1815 phrenology already appeared as an anti-establishment ‘breaker’ of systems; as an anti-academic, scientific de-mystifier of idealist philosophies; as a materialistic *bete noire* of spiritual religions; and as a deviant new body of neuro-anatomy. Although Scottish phrenology clearly developed in response to local social and cultural conditions, its initial image in the Edinburgh of 1815 situated it quite clearly in a system of conflicting social interests (41).

Gall’s materialist phrenology could legitimize political claims and provide potential support for scientific revisionism; it posed a threat to traditional Kirk values, and appropriated ground traditionally claimed by the university professoriate. Thus Grant must have been aware that he was unlikely to receive establishment approbation for approving Gall’s anatomy. He was challenging his mentors by telling the Medical Society in 1814, the year he became its President, that Gall

has done what lies within the power of man, to

forward our knowledge of the structure of the Brain: but he has been rather unfortunate in having his discoveries first announced by those who were unable to comprehend them, and in such a manner as rather to excite ridicule than interest the attention of the public. It has been frequently remarked, and not more frequently than justly, that the human constitution is ill calculated to stand any sudden change. Changes that are to be permanent are affected gradually and a period must always elapse before new discoveries can be said to constitute an addition to the stock of human knowledge. He, who, like Gall, makes a sudden bound forward in the career, anticipates the work of time. His enemies are violent, and his admirers enthusiastic (42).

There is no evidence that Grant accepted ‘bump’ diagnosis; from his statements it appears that he was principally impressed with the phrenologist’s *neuroanatomy* (43).

What Gordon considered “*thorough quackery*” Grant accepted as an eloquent demonstration of probable anatomical truths. For Gordon at any rate esoteric anatomy was tied to definable ideological positions, so perhaps Gall’s work had an equal and opposite philosophic appeal for Grant. Knowing that he was to adopt an explicitly mechanistic and reductionist philosophy in later years, and ally, himself with secular reformers, one is tempted to see him applaud Gall and Spurzheim for their attempt to make man, body and soul, subject to a deterministic, law-bound nature (44). But one has to be inordinately careful with such extrapolations – all one can legitimately state from the manuscript evidence is that he was prepared to ridicule Gall’s anti-mechanistic *critics*. Nevertheless this essay gives us a baseline from which to work. It shows Grant’s sarcastic vein already in evidence. He pilloried the *Edinburgh* reviewer who saw nothing

in Gall and Spurzheim but “absurd theory”. And he interpreted the French response to Gall’s and Spurzheim’s demonstration of the fibrous nature of the cerebellum more positively than Gordon was prepared to do. Grant unleashed his sarcasm at the *Edinburgh* reviewer’s expense (undoubtedly aware that he was twitting Gordon), commenting that this demonstration in Paris

might have been believed even to this day by men, in other respects, very intelligent, had not a species of animal, engendered from the concourse of sloth & ignorance but not to be easily taken in by any slight of hand ... smelt the hoax by which poor Cuvier and the other members of the committee had been duped, and soon satisfied himself that the black art must have been practised upon the occasion by the two imposters. You will in vain look thro Cuviers System for this newly discovered, quick-scented animal. It is almost indigenous in this island, and altho it seems to possess many characters of the Bimanum, it differs most essentially from the higher orders of animals in the small proportion which its organ of reflection bears to the nerves proceeding from it – a character which Soemmering and Ebel have discovered to be an index of Mental degradation. But to what extent this characteristic is applicable we shall have an opportunity of judging when we are in possession of that display of intellect which one of these nameless creatures has promised us in ... the last Number (XLVI) of the Edinburgh Review (46).

While Grant undeniably had an uncompromising nature I think we should beware of seeing this “Essay” as simply an early expression of his idiosyncratic temperament. Certainly personality factors might account for its *tone*. But in this he was not unique; a cutting tongue was characteristic of Scottish contemporaries like Fleming and Knox, the latter putting his stiletto of a pen to good use writing “slashing articles for a Radical print, condemnatory of parsons,

politicians and tuft hunters” (47). Better, before we settle on *psychological* explanations for Grant’s scientific preferences that we investigate the materialistic social milieu: the heterodox mechanistic views prevalent among Edinburgh graduates, those without academic or social positions and holding left-of-Whig political views in this period of post-Napoleonic social differentiation (48).

Geoffroy’s Transcendental Anatomy: Philosophic Deism and the Parisian Input

The tension between orthodox Presbyterian mentors and their materialist students in the Regency translated into determinate scientific disagreements. Within the university these did not focus primarily on phrenology, which remained largely a middle-class, anti-academic mercantilist movement. At the medical school Geoffroyan morphology and Lamarckism were more relevant issues. As a result of the practice of Edinburgh graduates finishing their medical education in Paris transformism and philosophical anatomy were quickly imported into the Scottish capital, and with these new doctrines came increasing discussion of philosophic deism. Almost all of the graduates whom we treat finished their medical education in Paris: Grant first went over in 1815-16, after Waterloo; Knox arrived in 1821; Browne spent some years in France after taking his diploma in 1826; and Coldstream suffered his spiritual crisis in Paris in 1827 (49). Knox recollects

that his first trip to Paris had perhaps the most dramatic influence on his life.

Probably the same could be said of Grant. With the restoration of peace, Grant was one of a number of students, including Thomas Addison (1793-1860; MD Edinburgh 1815) (50), able to spend the winter session 1815-16 at the Ecole de Médecine and the Jardin des Plantes (and, as was usual for a newly-qualified doctor, in touring the hospitals of the city). At the Jardin Grant attended the lectures of Henri de Blainville, Faujas St. Fond, and Abbé Hauy, and at the Institut he heard Georges Cuvier, founder of the “Macedonian Empire” of comparative anatomy (51). For five years Grant used his inheritance to travel back and forth through Europe. The summer of 1816 was spent in France and Italy, and nine months of 1816-17 in Rome; always appreciative of Italian science and culture, he studied comparative anatomy under Cuvier’s appointee Dr Metaxa in the reorganized College of La Sapienza. He devoted 1817 to investigating the marine invertebrates of the Mediterranean shores. That year he revisited Paris, as he did again in 1820, spending the intervening years and the summers of 1820-2 in the German states (52). He is said to have crossed the Alps seven times “and walked alone many thousands of miles through Europe, before 1820”. But it was to Paris that he returned frequently during the lifetime of his “kind patron” Cuvier (53) and until the deaths of Geoffroy (d.1844) and Blainville (d.1850), taking advantage of his “unlimited access” to the Muséum.

Discussing the role of ‘patronage’ in French science, Dorinda Outram notes that it helped shape careers, guaranteed the recipient’s worth, and produced strong loyalty (54). Grant admittedly traded on Cuvier’s patronage but he showed little ultimate fidelity to Cuvierian principles and became a lifelong supporter of Geoffroy and Blainville, suggesting perhaps that in his case it amounted to little more than the opening of doors. He frequently cited Cuvier’s works in his lectures to the Medical Society and in his Inaugural Dissertation (1814); in 1822 he completed an abridgement/translation of *Le Règne Animal* (never published); and in 1830 he reviewed Cuvier’s life and works in the short-lived *Foreign Review*. But while he talked respectfully of Cuvier’s “comprehensive genius”, he quietly criticized him for being “fettered by his earliest views of classification” (55): i.e., for insisting dogmatically on discrete *embranchements*. Grant had long since shifted to a Geoffroyan position. By 1830 he was already looking “for some more uniform and philosophic principles” according to which nature might be arranged into a taxonomic continuum, allowing Geoffroy’s “unity of composition” to stretch from monad to man – a continuum that was the very backbone of the transformists’ case. It is impossible, with the loss of Grant’s letters, to date his first meeting with Cuvier. Evidence from the later period shows them on familiar terms. From Richard Owen’s notes of his trip to Paris in 1831 we learn that Grant

attended Cuvier's *soirees*, and that he introduced the young Owen to the assembled dignitaries, including Georges' brother, Frédéric Cuvier (56). Actually, it is difficult to tell when Grant became familiar with *any* of the professors – indeed, whether he ever met or heard Lamarck lecture at all. He did possess Lamarck's *Extrait du Cours de Zoologie du Muséum d'Histoire Naturelle, sur les Animaux sans Vertèbres* (1812), but whether he attended Lamarck's course after 1815 is a moot point. Any attempt to determine Grant's acquaintance with Geoffroy is doubly hampered by the removal also of Geoffroy's letters by his family (57). At least by the late 1820s Grant knew Geoffroy well, and he collaborated with him in 1829 on the contentious subject of the generation of *Ornithorhynchus* (a subject closely connected with transformism – see Ch. 6). He backed Geoffroy against Cuvier in the Academy debate of 1830 on the mollusc-vertebrate analogies. And I will suggest (ch. 4) that Grant's importance as a London teacher was to instill Geoffroyan principles into a succession of students and urge homological arguments against Cuvierian functionalists, to the extent that by 1836 Geoffroy could hail Grant as “le premier entre tous les savans” and the “master” of philosophical anatomy (58).

In the 1820s, as Camille Limoges has reaffirmed, the Muséum was in the midst of a Golden Age in terms of international prestige, financing, and academic excellence (59). When Knox arrived in Paris in 1821 – after four years in medical service with the British Cape Town garrison –

there were already 30-40 British (mainly Scottish) medical students in the city, attracted by the lectures and hospital facilities (60). (Whereas private dissecting schools in Britain were always short on cadavers, the public Ecole de Médecine, visited by both Grant and Knox, had a plentiful supply.) Knox first saw Cuvier and his illustrious opponent, Geoffroy, in 1821. He later claimed to have been “in almost daily conference with Geoffroy” at this time (61). The morphologist had apparently ceased to teach, “having become wholly unintelligible to the students” on account of his transcendentalism. Nonetheless Knox was greatly impressed with Geoffroy’s philosophical anatomy, which he claimed would rival Newtonian physics in stature (62). He defended Geoffroy in Cuvier’s presence – indeed he considered the morphologist “a man of genius and original powers of thought, beyond the logical mind of the celebrated author of the “Ossemens Fossiles” (63). Knox never escaped Restoration science. As late as 1852 he admitted that his opinions had not altered since 1821, and that all his subsequent work had been an attempt to define homological relations in a rigorous way. Like a number of other Edinburgh-educated reformers (e.g. Symonds (64)), looking back from mid century, he saw precious little that was new or worthwhile in science after 1830 (including Darwinism).

Toby Appel has investigated the methodological split and institutional politics leading up to the public clash between

Cuvier and Geoffroy at the Academy. On reading her account one begins to appreciate why Scottish secularist reformers like Grant and Knox should have become so enamoured of Geoffroyan science. Geoffroy was conscious of his originality and as if to suggest that his new morphology presaged sweeping changes prefaced his *Philosophie Anatomique* (1818-1822) in a polemical style of contemporary social reformers (65). Whatever Cuvier's reservations, Geoffroy provided an attractive programme and initially enjoyed widespread support. In the book he had attempted to solve a long-standing puzzle by identifying the mammalian inner ear ossicles as the homologues of the opercular bones of fishes and his work was followed by a welter of homological studies in the 1820s on insects, crustaceans, and vertebrates. About the time of Grant's and Knox's first visit, J.-C.-L. Savigny was identifying the homologies of the appendages and mouthparts of insects and crustaceans; P.-A. Latreille and Victor Audouin in 1818-20 were seeking homologies between insect and crustacean exoskeletons; Latreille in 1820, sympathetic to a Lamarckian animal series, investigated continuities between crustaceans and fishes, and in 1823 began looking at cephalopod-fish homologies. (He was to drop this line of inquiry for fear of losing Cuvier's patronage.) Finally, in a series of controversial memoirs, Geoffroy in 1820 compared the anterior segments of insects with vertebral elements of the vertebrate skull. So there was great attraction in transcendental morphology and one can understand Grant's and Knox's eagerness to import the 'unity of plan' and theory of analogies

into Britain. As Geoffroy presented it, this was the most up-to-date and potentially reformist theory of anatomy.

Knox once recalled that his own lectures on unity of organization from 1824 “made all reflecting men acquainted with the fact that a new philosophy had appeared.” More modestly he records elsewhere that he explained Geoffroyan principles in three courses delivered in 1825-7 (66). Geoffroy’s deism and his attacks on vitalism and support for Lamarck from the mid-1820s (67) were congenial to the interests of Scottish medical reformers. In France however the growing royalist reaction was to lead to a falling off of support for Geoffroy. He had associated himself with German *Naturphilosophie*, long reviled for its excesses, and now seen to have anti-clerical and liberal overtones (68). With the growth of ecclesiastical power and Ultra rule following the ascent of Charles X, Cuvier’s attacks on Geoffroy became increasingly politically edged. If anything, the political reaction in France hardened the Edinburgh men against Cuvier. Knox was already prophesying difficulties for Geoffroy at the time when “Louis the Fat and Gross festered and rotted in the Thuleries; [and] the priests were gradually acquiring their lost influence”. Knowing his hatred of the Church and orthodoxy one cannot wonder that he, even more than Grant, criticized the Protestant Cuvier for exploiting his position in the conservative Academy to suppress Geoffroy’s views.

Platforms for Scientific Expression In Edinburgh 1820-7

Knox was to become renowned for his “savage radicalism”, and his hatred of “orthodoxy and Oxford” was only surpassed by his dislike of low transcendentalists who lacked the courage of their convictions (619). Grant was advocating Lamarckism as early as 1826. Later in London his lectures were conceived within a mechanistic and deistic framework, and he was to associate with Radical reformers. Yet throughout the 1820s both were socially and scientifically successful. Even before leaving for abroad Grant had been elected President of the Royal Medical Society (f. 1734), as Knox had taken the Chair of the Royal Physical Society (f. 1771). (These were well-endowed student societies, encouraged and aided by the university professorate (71).) In the 1820s Knox revealed himself a talented comparative anatomist, and Grant an exceptional invertebrate zoologist. Encouraged by his mentor Jameson and ally on invertebrate matters, Reverend John Fleming (1785-1857), Grant began an investigation of Scottish sponges, which resulted in a series of seminal articles in Jameson’s *Edinburgh Philosophical Journal*. By the mid-1820s Grant and Knox were playing leading roles in the newer student societies, Jameson’s Wernerian Society (f. 1808) and Plinian Society (f. 1823). They were also successful in seeking institutional and paying positions in the period 1824-6, and began publishing extensively on zoology, both in Jameson’s journal and David Brewster’s *Edinburgh Journal of Science*, which Grant is said to have

conducted for a while (72).

So the radicals gained public forums and the respect of the anti-mechanists they were to oppose (and, in Barclay's case, institutionally succeed). Now, the support of such a formidable combination of orthodox Presbyterians as Jameson, Brewster, Fleming, and Barclay for a deistic Lamarckian like Grant is problematic and requires careful handling within the confines of modern historiography. True, Grant's patrons differed among themselves. There were personal frictions. Brewster was piqued that Jameson persistently obstructed his attempts to examine the Museum collections (73); while Barclay was said to have been "the only man who was permitted to *lecture* Fleming without calling forth a *tart* reply" (74). On methodological grounds there were graver differences. Fleming's quasi-actualistic (75) and Scriptural principles set him in anti-catastrophist opposition not only to the Oxford diluvialists, but also to Jameson, who was "not disposed to give up the Mosaic Deluge – even geologically considered". As a result Fleming found him "irregular, cold, and distant" (76). Yet all four held uniformly high views of Grant's abilities and encouraged him at every opportunity. This despite the fact that Jameson must have known of Grant's predilection for Lamarckism (after all, he published Grant's exploratory Lamarckian papers), and probably Fleming (77) and Brewster knew of it too.

Looking at individual cases makes the problem no easier. Take that of David Brewster (1781-1868), Scotland's polymathic rival to William Whewell. J. H. Brooke depicts Brewster as a conservative Evangelical, whose theology was so "severe in its orthodoxy" that latitudinarians like Baden Powell could accuse him of narrow literalism (78). It is difficult to imagine Brewster condoning any aspect of Lamarckism. He was, after all, no less vehement than Cambridge contemporaries in his denunciation of Chambers' *Vestiges*, which he thought calculated to "degrade the godlike race" (79). But we should be careful in jumping forward a generation. By the 1840s anti-transformism in Britain had become a consistent and clearly defined ideology which – importantly – served a mediating function between rival theological schools*. No clearly formulated body of anti-

* Some sort of mediating structure was essential given the theological antagonism between Brewster and Whewell. Brooke has studied the cultural generation of Liberal Anglican and Evangelical Presbyterian natural theologies and sees the seeds of future disputes over plenitude and the plurality of worlds in the contrast between Brewster's view of 'Divine Resourcefulness' and Whewell's God of *Precision*.

But these religious cultures also supported distinct approaches to organic adaptation. Whewell insisted on a precise Paleyite fit between organism and niche. His argument depended upon limitation, on the assumption that organisms could not exist under other conditions; plants, for instance, would be destroyed by changes in day length, gravitation, and so on. Brewster saw the danger of basing natural religion on fallible science; what value Whewell's theology if savants one day proved that plants *could* adapt themselves? He accordingly offered a speculative counter-view. Even if this was only mooted to highlight the weakness of Whewell's positions it still presents an intriguing Presbyterian perspective on flexible adaptation and organic "accommodation". He argued that a *limitation* cannot be regarded as evidence of design. Quite the opposite: 'the *very want* of this limitation, or the

transformist knowledge existed in the mid-1820s. Possibly Grant's Lamarckism at this time had less meaning for Brewster. Even in his vitriolic assault on Chambers, Brewster admitted (in what is otherwise an enigmatic statement) that Lamarck had been an able naturalist who had produced a more complete system, adding: "We have seen *and admired* this handsome descendant of the Monads, dignified among his highly organized compeers, at the very time when he was elaborating in his latter days the ingenious speculation [of progressive development]" (82). I am not suggesting some latent sympathy, only that the contexts for discussion of the subject had not clearly evolved at this early time. With no broad-based anti-Lamarckian ideology – one that could serve Edinburgh, Cambridge, and London savants alike – Brewster had no preexistent, organized body of empirical knowledge to draw on.

While it might force us to reconsider aspects of

existence of an elastic energy in organic bodies by which they could accommodate themselves to a residence on every planet in the system, might be held to be a proof of divine wisdom and power" (80). Brewster was not deliberately underpinning some sort of *transformist* 'accommodation'; his 'energy' was presumably not elastic enough to transmute a species. But the episode does give a tantalizing glimpse into the potential of a Presbyterian theology of Divine Resourcefulness to do more heretical work. While there is no direct evidence that Grant himself alerted Brewster to the necessity of admitting organic change, it is interesting that in the same review Brewster does cite Grant as an authority on *infusoria*, while attempting to weaken Whewell's belief that the microscope had revealed "the finity of animal life" (81). Whatever the ultimate irony, Brewster's religion probably did place him closer to Grant than Whewell; for while he might disavow Grant's extreme formulation, there is no doubt that both conservative and radical could derive support from the same Calvinistic theology of organic accommodation.

traditional historiography, particularly as concerned with transmutation in the 1820s, Brewster's support for Grant cannot be doubted. He supplied the young zoologist with specimens (83), took seven of his papers between 1827 and 1829 for the *Edinburgh Journal of Science* (mostly on parasites and generation of zoophytes), commissioned his article "Zoophytology" for the *Edinburgh Encyclopaedia* (84), cited him as an authority, and recommended him for the zoology chair at London University. And when Brougham nominated Grant in 1827, Brewster wrote assuring him: "The Election of Dr Grant is most gratifying to me, and I am sure you will never have occasion to regret the action you have taken in his appointment" (85).

Explaining the patronage of other established teachers is no less challenging, particularly as Grant cemented a number of apparently incongruous alliances as he began his social climb. Similar interpretive problems attach, for example, to his relationship with the anti-mechanist extra-mural teacher John Barclay (1758-1826). One might have imagined *a priori* that Grant and the erudite but unbending Barclay would have become anything but friends. Would not Grant's ridiculing of Gordon's attack on Gall have betrayed him in Barclay's eyes? After all, Barclay saw physiology damn craniological speculation. He finished his comparative anatomy lectures by insisting that true science proves

hostile to ... the overweening conceit of the sceptic, who, with an intolerable degree of bigotry,

frequently talks of established laws, as if all the various secrets of nature were unfolded to him, and he were the confidant of the Sovereign of the Universe (86).

This attack on the materialism of medical men obviously cut to the heart of Grant's Lamarckian naturalism. Indeed this could have been one of the "idle hypotheses" which Barclay saw stemming from an imperfect acquaintance with fossil zoology (87). Barclay was a licenced minister and sat for many years in the General Assembly of the Church of Scotland. As his assistant George Ballingall testified, he was a lifelong anti-mechanist, and his *Inquiry into the Opinions, Ancient and Modern, Concerning Life and Organization* (1822), was calculated to check the growth of secularism (88). But ironically Barclay had posed the questions that were to intrigue Grant and Knox in their heterodox ways. The *Inquiry* was ostensibly aimed at atheists who advocated self-existent laws "antecedent to all legislative authority", and chemical reductionists who "ransacked the regions of fancy" rather than admit soul and deity (89). Central to Barclay's anti-atomistic programme was a challenge to materialists to explain "How come these structures [of animal and plant life] to be organized"? Confident that medical materialists were ultimately unable to answer the fundamental questions of organization, he threw down a challenge at the close of the *Inquiry*: "those physiologists who are inclined to favour materialism have never attempted to explain how the first parents of the different species of animals and plants might possibly have been formed". Curiously, for a detailed

treatise chronicling even the most extravagant doctrines, Lamarck was never mentioned. Yet the *Inquiry* was obviously incompatible with the mechanistic self-sufficiency of Lamarckism. Barclay and young Grant also steered diametric courses on specific issues. Barclay in the *Inquiry* warned of the dire consequences of physico-chemical reductionism – of invidious theories of infusorial origins in molecular aggregation, the sort which would eventually make man himself “a species of chemical compound” (90). For this he took to task J. B. Fray and “the celebrated Darwin”. Yet by 1825 Grant was already favourably disposed towards spontaneous generation (91) and he remained a committed reductionist, enamoured of French determinism and Darwin’s *Zoonomia* alike (92).

These seem insuperable difficulties, yet Grant and Barclay were professional colleagues and in personal contact for six years, 1820-6 (although Barclay died just before Grant published his Lamarckian speculations in Jameson’s journal). Grant entered Barclay’s class on returning from the Continent in 1820 (93). He was diligent and seclusive, and the results were evidently impressive because at the onset of Barclay’s illness in 1824 the invertebrate part of his course was delegated to him. Great distinction attached to teaching in Barclay’s extra-mural school and the experience was to stand Grant in good stead (94). In 1825 Knox himself was taken into partnership with Barclay, and the following year became his successor. Thus these men who “breathed a doubting Theism”

and who saw development and transmutation as a sequel to the law of unity dominated the most popular extra-mural school in 1824-6. Attendances remained high. In 1826-8 (for which figures are available) the size of Knox's class was almost twice that of his nearest rival (95). Thus the new morphology and "despised philosophy of St Hilaire" (96) reached a large audience in Edinburgh. This goes a long way to explain why those who, like Symonds, had been students at this time looked back from the 1860s on the "transcendental Anatomy of Oken, and Serres, and St. Hilaire" as one of the major developments of science (96).

Another of Grant's associates, the surly but incisive Minister of Flisk Reverend John Fleming (1785-1857), showed no reticence in tackling Lamarck. Undoubtedly he was drawn to Lamarck's works, especially the "valuable" (98) *Histoire Naturelle des Animaux sans Vertèbres* (1815-22), for the same reason as Grant – because Lamarck, as Professor of the parts of creation no one much cared about, "insects, worms, and microscopic animals", wrote prolifically on zoophytes. Since Fleming and Grant both studied Firth of Forth invertebrates, Lamarck figured predominantly in their writings (he is one of the most frequently cited authors in Grant's papers from 1825-9). Because of this shared interest Fleming and Grant became colleagues and friends (99). Fleming presented Grant with Shetland zoophytes and over thirty species of sponges for examination (100). By the time Grant reached London, Fleming too was citing him as an authority (101), and in the

History of British Animals (1828) coined the name *Grantia* for the genus of calcareous sponges “to commemorate his valuable services in elucidating the physiology of sponges” (102). Unlike many English zoologists, who believed that Lamarck could count on no support at home, Fleming knew that he had “succeeded in making some converts” (103). If, as seems likely, Fleming was aware of Grant’s transformism, it could have had important repercussions, which we can better appreciate in light of Bartholomew’s revisionism (see Ch. 5). According to Bartholomew, Lyell’s motives for moving to a non-progressionist palaeontology could have stemmed from his hatred of Grantian-like transmutation. But *Fleming* too between 1822 and 1829 moved away from progression. In the *Philosophy of Zoology* (1822) he countenanced an ascending fossil record, whereas by 1829 he was arguing against progressive development (104). Could it have been coincidence that in the intervening years his young friend had given the progressionist edifice a Lamarckian meaning? Whether or not Grant contributed to these second thoughts, Fleming did go on to deploy retrogressive fossil sequences, and in the Tory *Quarterly* wield Occam’s razor to whittle away at Lamarck’s argument, much as Lyell and Whewell were to do (105).

The theory professedly advocates the simplicity of Nature’s proceedings, and yet the method prescribed to her for the formation of Man is the most complex and circuitous imaginable. Indeed, the whole scheme, as an exposition of the plan of procedure, is so obviously a dream of the imagination, that one may well be surprised to find it occupying a

place in the records of science. It is true, that the prejudiced advocates of this system have imagined that the physical distribution of petrifications gave support to their views. They have announced as an established truth, that the relics of animals, imbedded in the oldest rocks containing organic remains, belong to the simplest kinds, such as the zoophytes; and that the newer rocks exhibit relics of animals progressively advancing to the most perfectly organized structures. When, however, we open the cabinets of Nature, and examine the stores of her earliest works which she has preserved, we find in the same drawer of greywacke, transition limestone, or old red sandstone, the relics of zoophytes and molluscan along with the bones of *vertebrated animals*, and the stems of *dicotyledonous plants* (106).

Fleming's interest, like Barclay's, was avowedly vitalistic, and he was worried by the growing materialism of Edinburgh medical men (the extremity of his opposition might be gauged by the fact that he even denounced Cuvier's law of correlation as contributing to "the clumsy fabric of modern Materialism" (107)). Yet Fleming's opposition to Lamarck was far from total; he was indebted to Lamarck for his taxonomy based on neurological criteria, his vertebrate/invertebrate distinction, and dichotomous system of classification (108). In the *Philosophy* he praised Lamarck's invertebrate zoology and was not as derogatory of French transformism as he was later to be (after Grant had made known his position). Acknowledging that younger rocks housed more modern faunal types, he wrote:

Attempts have been made to account for these circumstances by supposing, that the present races of animals and vegetables, are the descendants of those whose remains have been preserved in the rocks, and that the difference of character may have arisen from a change in the physical constitution of the air, or the surface of the earth, producing a corresponding change on the forms of

organized beings. The influence of cultivation on vegetables, of domestication on animals, and of climate on man himself, may be considered as strengthening this conjecture. But there are several difficulties which present themselves to those who adopt this opinion. The effect of circumstances on the appearance of living beings, is circumscribed within certain limits, so that no transmutation of species was ever ascertained to take place; and it is well known, that the fossil species differ as much, nay more, from the recent kinds, as these last do from one another. **It remains, likewise, for the abettors of this opinion, to connect the extinct with the living races, by ascertaining the intermediate links or transitions.** The task, we fear, will not be executed speedily (109).

Grant was rarely one to annotate. But in his copy of Fleming's *Philosophy* he did put a cross against this penultimate sentence (in bold above). The identification of intermediates would prove an important part of Grant's future programme: from 1826 until 1853 he was to draw transformist conclusions primarily from the evidence of fossil and recent continuity.

So Fleming dissociated himself from continuity and progressive development in the 1820s. He insisted, for example, that the gap between vertebrates and molluscs was unbridgeable (a fact contested by Grant) (110). Nonetheless even in 1829 he was willing to concede that study of progression had some heuristic value, believing that, "if used synthetically, to communicate knowledge previously acquired, it will be found greatly to assist the student in forming accurate conceptions of the relations of different organs, by exhibiting the transitions from their simple to

their complicated states" (111), and he took as his example a branching taxonomy of sponges derived from Grant's researches (see below).

So the relationship between Fleming and Grant was complex. It was never a simple matter of Grant's transformism and Fleming's reaction, for both seem to have had Lamarckian proclivities (in the taxonomic, rather than narrowly transformist sense). Perhaps the best way to understand the similarities of their geological philosophies is from the standpoint of their common Presbyterian culture. Say we look at the Minister of Flisk anew, concentrating on his *evangelism*. Now, for a Scottish evangelical, *revelation* supported, defined, and set the limits to natural theology (112). Thus when Fleming came to criticize Buckland's catastrophic interpretation of the Flood in 1826, he stood securely on *Scriptural* grounds. He entered the debate "as one deeply interested in the authority of revelation, although he was not of course indifferent to the progress of geological science". He pointed up the inconsistencies between the Mosaic account and Cuvier's and Buckland's geological reconstructions, and proposed an alternative nonviolent interpretation based on Scripture:

the simple narrative of Moses permits me to believe, that the waters rose upon the earth by degrees, and returned by degrees; that means were employed by the Author of the calamity to preserve pairs of the land animals [allowing the continuity that Buckland denied]; that the Flood exhibited no violent impetuosity With this conviction in my mind, I am not prepared to witness in nature any

remaining marks of the catastrophe, and I feel my respect for the authority of revelation heightened, when I see on the present surface no memorials of the event (113).

Geologically speaking, the Flood could be dismissed as an invisible overlay – as an event that made no impact on the fossil record. Evangelical gradualism could in the same way have encouraged Grant in his search for theories of palaeontological and taxonomic continuity. From Fleming’s evangelical perspective, it was *catastrophism* that would lead to infidelity, for if Buckland’s creed be true, “then must the Book of Genesis be blotted out of the records of inspiration”. One should be cautious of interpreting Fleming’s response as *wholly* Scriptural; and I only draw out this aspect to make a point – in fact he spent considerable time refuting the geological evidence of a violent inundation. Nevertheless he was encouraged by his Scripturalism to adopt a gradualist geology and seek an uninterrupted fossil record (for which reason we should beware seeing him ‘anticipate’ Lyell’s actualist principles (114) – Lyell’s radical actualism and Fleming’s evangelical anti-catastrophism reflect different cultural ideologies). Non-violence was not of course unique to evangelicals, and until further studies are carried out it remains uncertain how prevalent it was even among them. It is telling, however, that another prominent minister, Thomas Chalmers, also independently rejected Cuvier’s violent interpretation (115). The point is that a committed gradualist like Grant, for whom “Nature knows no sudden transitions” (116), would have been

among those who assured Fleming that he had dealt the “deathblow to the diluvial hypothesis” (117). Like the rest of his generation, Grant was immensely impressed by Cuvier’s fossil labours, yet he eschewed all cataclysmic theories, and the prevalent evangelical distaste for geological turbulence may help to explain why.

Fleming’s views were widely known in Edinburgh. He read his anti-catastrophist paper to the Wernerian Society on 25 March 1826, when Grant was on the Council (118), and Jameson subsequently published it. Strong ideological differences admittedly existed between Grant and Fleming, and the two disagreed on specifics, e.g. on the value of the evidence of a formerly warmer Arctic (which Fleming disputed, but which was crucial to Grant’s transformist model (119)). Yet on issues of invertebrate taxonomy, Lamarckian conchology, ‘lineage’ models, and gradualist geology, they were in perfect accord. Indeed, Fleming’s ‘stem and branch’ models of sponge affinity (see below) were based on Grant’s findings – findings which were originally embedded in a transformist context. To understand that context we will now examine Grant’s *zoological* work and its ideological framework.

The Wernerian Natural History Society Grant, Sponges, and Transformism

The Sponges, which have long occupied the attention of naturalists, and given rise to considerable difference of opinion regarding their true place in the System of Nature, have at length been examined

by an observer possessing the requisite leisure, opportunity, industry, and talent for conducting such intricate researches. I here refer to the papers which have appeared in the Edinburgh Philosophical Journal by Dr R. E. Grant, now Professor of Zoology in the University of London. He has succeeded in determining the functions of the pores, and the origin and mode of development of the ova.

Fleming in his *History of British Animals* (1828) (120).

This section returns to the study of scientific platforms available to the new materialist zoologists. So far we have only discussed Barclay's extra-mural course; this allowed Grant to systematize his invertebrate zoology and Knox to expound his ideas on the higher anatomy. But there was competition for other positions at this time. in December 1824 Knox was appointed by the College of Surgeons of Edinburgh at £100 p.a. to enlarge their pathological museum, to which end he purchased Charles Bell's anatomical collection from the Great Windmill Street Museum. When the post of Conservator to the museum was advertized at £150 p.a. in May 1826, Knox encountered local opposition to his candidature and, after some politicking, a rival faction championed Grant's claims. They considered Grant "equally, distinguished for his abilities and zeal in his profession and for the utmost urbanity and modesty of manner" (121). As the resident, Knox was elected by a wide margin; but the episode does suggest that Grant was now casting around for a paying position. (His loss, incidentally, did not prevent his continuing to support Knox, even through the Burke and Hare crisis (122).)

Paid employment obviously provided more than financial security (although as extra-mural lecturer and Conservator to the College Knox was beginning to do well for himself). Institutional support conveyed scientific respectability. Hence Grant, after relinquishing Barclay's course to Knox, continued to lecture at the Northern Institution (123). But the main forum in the 1820s for original papers on zoological subjects remained Jameson's Wernerian and Plinian Societies. These were useful to naturalists like Grant and Coldstream working the Firth of Forth estuary (124), and to Jameson's assistants in the museum dealing with specimens received from the colonies. In this capacity Knox, who joined the Wernerian in 1821 and was elected to the Council in 1823, read memoirs on the *Ornithorhynchus* and cassowary from New Holland (125), while Grant read papers on the Australian marsupial *Parameles* and Brazilian Paca (126). Grant was said to have been Jameson's "constant pupil" from 1820-7 (127). He spent the autumns 'zoologizing' around the Scottish coasts, and we know that he continued to attend lectures because we possess his MS notes of Barclay's course in 1821 and Jameson's for 1823 (128). Jameson produced innumerable protégés of the first calibre, and it is interesting that he continued to support Grant whom he presumably knew to be a transformist. He was still asking Grant's advice on specialist matters long after he had left for London (matters like that of the suspected *Ornithorhynchus* egg, of intimate concern to transformists

(129) – see Ch. 6). Once again, one can only conclude that transformism, while it was dismissed or denounced by most, was nonetheless not as politically or socially threatening in Presbyterian Scotland in the mid-1820s as it was to be for English Anglicans during the troubled Reform years.

From a study of Grant’s papers we can reconstruct the invertebrate context of his transformism at the Wernerian Society. He was prolific, publishing twenty papers on invertebrates alone between 1825 and 1827, in the process describing half a dozen new species of sponges (130). Like Knox, Grant was proposed for the Society by Jameson in 1821. But he submitted nothing until February 1825, when according to the Minutes he read the first of the fifteen papers that were to be presented over the next two years, mostly on Firth of Forth invertebrates: sponges, cephalopods, the generation of zoophytes and their free-swimming ova. This was Grant’s most productive period. The Wernerian Society remained his preferred platform and he was elected to its Council in 1825 and 1826 (131).

Grant’s researches on sponges – the work for which he is largely remembered – will be considered here primarily from the perspective of his speculations on transformism. That is, we will examine these researches with one eye to the *use* to which he put them. We are fortunate that most of the memoirs mentioned in the minutes were published, thus we can actually reconstruct his taxonomic base for Lamarckian continuity.

Later Victorians like J. S. Bowerbank (132) acknowledged Grant's pioneering work and continued to regard him as an expert on sponges as late as the 1860s (133). But attempts to describe Grant's achievements in a few words have inevitably lead to over simplification; they also run the risk of attributing to him assumptions common to the age because he routinely incorporated them into his papers. I. B. J. Sollas early this century acknowledged that "The most important of his contributions was the discovery that water enters the sponge by small apertures scattered over the surface, and leaves it at certain larger holes, always pursuing a fixed course" (134). While R. B. Freeman has suggested that it was as a result of his "fundamental researches on the structure and functions of sponges" that "their animal nature was first properly understood" (135).

Attempts to isolate a single 'discovery' by which his name might be associated tend to sustain a positivist image no longer fashionable. Today it is more historiographically relevant to ask what assumptions he shared with his contemporaries, why he guided his research along specific lines, and, most important, to what use he put his new formulations. On the first point we should note that there was already a strong Continental tradition of zoophyte studies with which he was quite familiar; his own papers with their persistent citations of foreign authors undermine the old heroic image. Grant saw himself as part of a larger

‘community’ which comprised numerous savants, including Lamarck and the polyp specialist Jean Lamouroux (1779-1825) in France, the coral expert A. F. Schweigger of Konigsberg (whom Grant knew), and zoologists Fleming and George Montagu at home (136). Communications networks were again well established after Napoleon’s defeat, and savants in Edinburgh, Paris, and Konigsberg could quickly familiarize themselves with one another’s work. They shared a set of assumptions, one of which by the 1820s was that sponges were animals. Montagu acknowledged that it was “pretty generally allowed, that they are truly of an animal substance” (137). One can of course find exceptions, but even when an extreme view was taken, such as J. E. Gray’s in 1824, that they should be removed to the vegetable kingdom, Thomas Bell was quick to reinforce the conclusion of the “most accurate and observing naturalists” that sponges “possessed a true animal structure” (138). So Grant, in his first long paper on the structure and function of sponges (read to the Wernerian in February and March 1825, and published in four parts in Jameson’s journal), assumed throughout that he was dealing with animals. Nor was he first to notice the currents. These had been intermittently recorded, for instance by Ellis and Schweigger, and as recently as 1824 Thomas Bell had observed sea water being “alternately, sucked in and expelled” from the “mouths of the tubes”, even though he had no idea what caused it (but assumed some motion of the gelatinous substance) (1319). Grant’s distinction was to make the most systematic study of the canal structure. He established by observation

and experiment that a current was generated, though not as a result of any contractile property of the tissue, but probably from ciliary action (although even at magnification x100 he never managed to see the cilia). Water was drawn in through tiny, entrance pores, circulated through the canals, and expelled from larger fecal orifices, which were often elevated above the surface on papillae. By painstaking study of sponges *in situ* he noticed that the flow ceased if the pores were above the water line, even though those below continued to function normally. As happens with pieces of methodical research, Grant's became a reference point for citation – and this is the operative point. It was so definitive on all aspects of circulation that it was treated as a new beginning (140).

What to my mind is more interesting is that he made this circulation a defining characteristic of sponges. This then allowed him to tackle certain anomalous organisms, like the fresh-water *Spongilla friabilis*, from which he was to draw his transformist conclusions. *Spongilla* really was a contemporary problem. No one had then examined its internal structure (141), nor had any clear consensus emerged as to its plant or animal nature. He obtained specimens of this flat sponge-like organism from Lochend and disproved the wilder claims (that it was a vegetable, a nidus of some aquatic insect, or an agglutinated tubularia tube). Even though Lamarck and Fleming separated *Spongilla* from true

sponges, Grant supported Schweigger's view that they were related. Overcoming extreme difficulty, he examined its internal structure, taking successive horizontal sections for study under the microscope. He described the pores, dissected out the globular matter, and observed the fecal orifices. These were not raised on papillae, but were irregular and lacking protective spicula. He first observed the currents generated in specimens *in situ*, and was eventually able to detect them in aquarium specimens.

Despite a simpler structure, *Spongilla* in Grant's view bore a close resemblance to marine sponges, and it thus provided him with a base on which to build a graduated invertebrate series – one that would elegantly demonstrate the truth of ascent by progressive transformation. *Spongilla* by Grant's *definition* was a sponge, having separate entrance and exit pores, but of “more imperfect structure”. The fecal orifices were simpler, and the ova were less developed, lacking cilia or the ability to swim. Given these facts, he made appropriate palaeo-environmental assumptions in order to draw convincing transformist conclusions:

From this greater simplicity of structure, we are forced to consider it as more ancient than the marine sponges, and most probably their original parent; and, as its descendants have greatly improved their organization, during the many changes that have taken place in the composition of the ocean, while the *Spongilla*, living constantly in the same unaltered medium, has retained its primitive simplicity, it is highly probable that the vast abyss, in which the *spongilla* originated and left its progeny, was fresh, and has gradually become saline, by the materials brought to it by rivers,

like the salt lakes of Persia and Siberia (142).

So in these first papers Grant was already drawing palaeobiological and environmental conclusions on the basis of anatomical findings, an approach which characterized his work for the next quarter century. He concluded that *Spongilla*'s unprotected pores and lack of defence against grazing predators or invading insects obscurely points to

the unpeopled state of the waters of the globe and consequent absence of these numerous assailants, at the period of the first formation of this zoophyte; and its aptness for secreting silica, and the abundance of that earth in its skeleton, show the period of its creation to have been nearly synchronous with that of the siliceous or primitive rocks.

The assumption that *Spongilla* had existed in a largely “unpeopled” primeval freshwater environment, i.e. before the emergence of higher predatory forms, reflects his prior commitment to an inexorable serial progression in time. Again, the argument that the animal’s siliceous matter proved it to be coeval with the oldest stratified rocks was to be used repeatedly in future defence of his serial edifice.

Grant read his paper on *Spongilla* to Wernerian members on 11 February 1826, and throughout the year he continued to develop his graduated invertebrate series and reinforce the palaeo-environmental conclusions. At the 8 April meeting he revealed the way in which the series might be extended (143). Lamarck had placed sponges next to the more advanced polypiferous *Alcyonium*. Grant now interposed a new Scottish

genus *Cliona*, named for a fleshy zoophyte he had discovered growing inside oyster shells. It possessed spicula and was traversed by canals like sponges, and its ova were mobile. While fecal orifices projecting above the surface definitely related it to advanced marine sponges, what marked it off as truly intermediate were polyps “of extraordinary minuteness and delicacy placed around the margin of the orifice” (144).

Alcyonium is polypiferous, but sponges are not. Therefore *Cliona* presented the comparative anatomist with a “very, interesting combination of properties”:

it is closely allied to the *Alcyonium* by its contractile fleshy texture, and by its distinct though microscopic polypi; and it is allied to the *Sponge* by its siliceous tubular spicula, ramified internal canals, tubular papillae, regular currents, and the distribution of its ova. It differs, however, from the *Alcyonium*, in not presenting a free surface, covered with a coriaceous integument, marked with stellate pores for the lodgment of distinct polypi; and it differs from the *Sponge* in the obvious contractility of its papillae and general texture, in its possessing distinct polypi, and in its surface not being free, and covered with open angular pores. It constitutes a distinct genus, forming a connecting link between the *Alcyonium* and the *Sponge*, and throws much light on the nature of the latter zoophyte (145).

Grant investigated *Cliona* with one eye to the Lamarckian scale, manoeuvring into a position from which he could detail the subtle ‘transition’ from the primitive freshwater *Spongilla*, via marine sponges and the polyp-bearing sponge-like *Cliona*, to true polypiferous animals such as *Alcyonium*. Thus he was able to produce “a regular and beautiful gradation from this simple substance [*Spongilla*], to the most complex polypiferous zoophytes” (146).

He continued refining his classification of silicious and calcareous sponges to approximate more closely the ideal of an invertebrate continuum. Later in 1826 he sorted the silicious spicula into primitive and advanced designs. The primitive acuminated “vitreous spiculum” characterized archaic freshwater and the simpler marine sponges (147); and he went on to describe two advanced designs, suitable for more sophisticated functions in higher marine sponges and *Cliona*. Again he resorted to conjectural palaeobiological explanations for *Cliona*’s defensive spicula:

It may be supposed, that, at the time of its formation, animalcules of a larger magnitude swarmed in the heated ocean; and this stronger mechanical protection of the pores seems to have been the more necessary It is interesting to observe, that the earthy matter of the skeleton of these earliest inhabitants of the ocean, is the same with what we know to have paved the bottom of the vast abyss at the remotest periods we can reach of the earth’s history, whether we imagine the silica of the primitive rocks formed by the oxidation of the solid surface, or precipitated from the superincumbent fluid. The appearance of many of their crystalline siliceous pointed spicula is the same with that of the slender hexaedral acuminated prisms which silica naturally assumes in the crystallized state; and the silicious crystals formed by nature contain cavities and fluids like those formed by organic life. The laws, therefore, which regulate the forms of the simplest silicious spicula composing the skeleton of the marine sponge, do not appear to differ much from those which regulate the forms of brute matter.

In later years Grant’s reductionism was to become more pronounced, but even here, despite (or perhaps because of) a rigorous environmental determinism, he shunned the teleo-

logical emphasis of English divines. He ended his paper, not in praise of Divine beneficence, but with a more secular hymn – recommending this obscure part of zoology as revealing “new scones of infinite wisdom in the economy of Nature” (148).

Lamarck and Stratigraphy In Scotland

In the bones contained in the Plaster or Gypsum formation, we have the most ancient monuments of land animals that are yet known to exist, and on account of their great antiquity it is perhaps less wonderful that they resemble so little any of the animals now inhabiting the earth. The genera and species of animals that inhabit this globe are evidently subject to change; some are entirely extinguished. – As old species perish, do new species rise up? Is there some secret law of animal reproduction by which there is a succession of species in the course of ages, as there is of individuals in the course of years! – The mind is lost amid the uncertain lights and gigantic images that pan before it

John Playfair, the elderly Whig and Huttonian expositor, writing in the *Edinburgh Review* on the larger questions raised by Cuvier's and Brongniart's researches (149).

Roy Porter has conveniently contrasted the socially differentiated schools of geology in London and Edinburgh. The Geological Society of London (f. 1807) loudly trumpeted its Baconian inductivism. By formalizing a scheme of cooperative research, agreeing methodological constraints, and tacitly banning divisive theories of the earth, the predominantly Anglican managers after the anti-mineralogical ‘counter-revolution’ succeeded in avoiding the rifts which

dogged Scottish geology, while retaining the discipline on a ‘respectable’ footing. Whereas these gentlemen careerists eschewed all preconceived theory, Scriptural aid, and cosmological pretension (150), geologists north of the Tweed remained philosophical. As a consequence their discipline was rent with division, and not merely into the much-caricatured camps of geognostic Wernerians led by Jameson and Whig Huttonians headed by Playfair and Sir James Hall. Porter sees the Scottish schools distinguished by broader philosophical and religious allegiances as a result of the centralized ‘Common Sense’ tradition and the tightly-knit groups of the Edinburgh intelligentsia (151). Sharp rivalry was certainly characteristic of the period. Jameson’s Wernerians seceded from the Royal Society of Edinburgh in 1808, leaving it largely to the Huttonians (it was at the RSE, for example, that Grant remembered seeing Hall experiment on rock formation through heat and pressure (152)). A leading Whig like Playfair – liberal to the point of defending a deistic Huttonianism (153) and recommending the atheist John Leslie as his successor in the mathematics chair (154) – would snipe at Cuvier and Jameson from the brush of the *Edinburgh Review* (155). The controversialist Fleming relished a fight with Buckland over the Flood or with his colleague Conybeare over Arctic cooling (156). *Theories* of the earth with a constitutive moral element provided by Common Sense philosophy or Evangelical theology were thus not uncommon in Scotland: notice, for example, that Jameson retitled Cuvier’s

Discours Préliminaire in translation *Essay on the Theory of the Earth* (1813) (157).

And in casting broader philosophical nets, the Edinburgh geologists obviously intended to rope species into any final explanation. Jameson concluded his course on natural history in 1826 by discussing the “Origin of Species” (158) – which was not so much a taboo subject in London, but one which, because nothing of it was known, defied anything being said. Helped by this divisiveness and the failure of ‘theory’ to be outlawed by the imposition of methodological restraints, Lamarckism gained quicker acceptance among the medical materialists.

The first point to note about Lamarck in Scotland is that his *conchological* work was enthusiastically received. Numerous epitomes and extracts of the classificatory part of his *Histoire Naturelle des Animaux sans Vertèbres* (1815-22) appeared in translation. Works by such condensationists as J. S. Miller, E. A. Crouch, and Charles Dubois (159) were extensively advertised in the Edinburgh journals. Perhaps no greater testimony of Lamarck’s success can be given than to note the approval of a mineralogist like John MacCulloch. MacCulloch was notoriously insensitive to the new palaeontology, and often rude about French geology in general (160). (His father, a Brittany businessman, had lost everything during the Revolution and been imprisoned during the Terror.) Yet even he retained a healthy respect for Lamarck’s conchology while remaining antagonistic to the classificatory efforts of compatriots like Ami Boué (161).

The contextual generation of Lamarck's *biologie* has been studied by Jon Hodge, Richard Burkhardt, and Robert Richards (162), and it is not my intention to go into it here. I would only mention that Lamarck's transformism was not a deduction from stratigraphical palaeontology, nor did he set it into a geological context. In this respect he differed from Enlightenment contemporaries like the influential Senator, Bernard Comte de Lacépède (1756-1825), who appreciated the importance of Cuvierian fossil zoology (163). However the potential stratigraphical application of Lamarckism was quickly realized by critics and admirers alike. Cuvier in the *Discours* interpreted Lamarck palaeontologically, refuting his doctrine on the grounds of the lack of fossil intermediates, e.g. between *Palaeotherium* and living herbivores (164). Jameson in his English introduction to Cuvier's *Theory* considered Lamarck in a similar way. Since the *Theory* ran to five editions over fourteen years (1813-27), and all carried this introduction (165), one is left in no doubt that Lamarckism in the 1810s had become primarily a palaeontological issue. But this was only to be expected; even before the translation appeared, Playfair wondered what secret reproductive process could explain Cuvier's and Brongniart's fossil progression.

While the question tantalized liberal Huttonians, it assumed even greater relevance for the Wernerian-trained young. The alarmist responses of English geologists reveal the potential danger of Lamarck in a Wernerian stratigraphical context. G. B. Greenough in his *Critical Examination of the*

First Principles of Geology (1819) denied Wernerian claims that the age of rocks and scale of life corresponded, i.e. that lowly zoophytes are interred in the first formed rocks, shell fish in the next, and so on (166). His repudiation anticipated later reasoning on retrogressive fossil sequences – he instanced cases where the “first born” were of high genealogy, noting that the elevated nautilus was as old as “the madreporean polypus”, that fish had undergone little progress, and that crocodiles were an ancient group. Clearly such anti-progressionist strategies pre-date Lyell’s *Principles* or Fleming’s shift of position. However the threat envisaged by Greenough, Lyell, and Fleming only materialized with Grant’s work in the 1820s. For while at the Wernerian Society Grant’s transformism was an extrapolation from the living invertebrate series, in anonymous papers he did explore the wider cosmological and palaeontological applications.

It is generally assumed that the anonymous “Observations on the Nature and Importance of Geology”, published in Jameson’s journal in 1826, was authored by Grant. Nothing in this paper is inconsistent with his known views; and the reason for anonymity is plain enough – he was casting around for a paying position, and a few months later was to take the London chair. (Actually, anonymous papers were *routinely* inserted in the journal at this time, so it would not have attracted undue attention on that score.) In this paper he praises the “sagacious Lamarck (167) and introduces his

double series rising “in a gradual manner” from twin bases, the spontaneously generated infusoria and worms – an ascent wrought by the “operation of external circumstances”. Grant tests this living ‘scale’ against the fossil record and concludes that Lamarck’s theory was congruent with the *actual* history of life. He further supports the mutability of species on the grounds that a change of situation, food, and climate have been sufficient to alter our domestic livestock. This was not a radically innovative idea: the findings of horticulturalists and stock breeders had been invoked by Erasmus Darwin, P.-J. Cabanis, and others (168), but Fleming in 1822 still considered this the strongest evidence supporting the transformists’ case. Geoffroy, too, inclining more towards “metamorphosis” at this time, was also concerned with domestication, particularly the changes undergone by European livestock transported to South America (169).

Burkhardt believes that Lamarck’s transformism was an attempt to avoid the unpalatable fact of extinction. Although by 1826 Cuvier’s discoveries had placed extinction beyond question, Grant could still suggest that “many fossil species to which no originals can be found, may not be extinct, but have gradually passed into others” (170). Grant’s conclusion was “that the various forms have been evolved from a primitive model, and that the species have arisen from an original generic form.” But there is no mention of causation beyond “external circumstances” and, implicitly, ‘pressure’ from below as the “aggregation process of animal elements”

(i.e. spontaneous generation) pushed the chain upwards. But he does leave clues as to his larger environmental ideas. He challenges Fleming, for instance, by arguing that “many of the petrifications of cold climates, whose species and families are [now] produced only in hot countries, indicate a great change in the temperature of their former situations (171). To understand the kind of conventional dynamical theory Grant has in mind, we must turn to another – formerly unrecognized – anonymous article in the *Edinburgh Philosophical Journal* on the “Changes which Life has experienced on the Globe” (1827). This paper has the same naturalistic and anti-cataclysmic tenor as the rest of Grant’s work; more important, it provides an early pointer to his mature views on directional temperature change and progressive development (172).

As Philip Lawrence’s survey shows, belief in directional earth history ‘peaked’ about mid-decade, and dynamical explanations once more gained in popularity following Joseph Fourier’s studies on residual planetary heat, themselves rooted in Laplacean nebular theory (173). Writing in 1826-7, at a time of general consensus on igneous forces but before the founding of a geological dynamics based on central heat, Grant correlated the gradual migration of life away from the poles with the loss of uniformly-warm global temperatures and onset of climatic zoning. The presence of coal at high latitudes and wide distribution of fossil elephants bore

testimony to the original uniformity. Climatic zoning and the accompanying changes in temperature, tides, sea level, igneous activity, and atmospheric conditions provided “the regular, general, and continued natural causes of the modifications which life has undergone” (174). With this inexorable climatic shift came successive *gradual* changes of species:

new species appeared with new conditions of existence. But, in examining the series of fossil remains that are found buried in the strata of the globe, there is nowhere perceived a distinct line of demarcation between the different terms of that series, so as to prove that life has been once or oftener totally renewed on the earth. On the contrary, we discover in it a proof of the successive and gradual change which we have pointed out.

So the continued operation of “a small number of causes” resulted in a graduated fossil series exhibiting none of the breaks demanded by catastrophists. Like Lyell, Grant attributed to gradualism an *a priori* methodological correctness:

Our theory, which is founded on all the facts that have been established cannot but prevail over the systems hitherto proposed, for it is in harmony with the natural laws of order and permanency which rule the universe, and is, moreover, supported by the most accredited physico-mathematical theories; whereas those systems, founded upon perturbations of cataclysms, which science, facts and human reason equally reject, only increase the number of those imaginary conceptions which have been successively published for several centuries (175).

Grant was putting together a unique science from common contemporary components for definable ideological ends. Blending Lamarck’s escalator with a Wernerian fossil ascent, he was driving the system using available dynamical and palaeoclimatic forces. And like Lyell, who conflated the

principles of actualism and non-progressionism for ideological ends, Grant conflated lawfulness and gradualism in order to unseat Cuvierian catastrophism. Oddly his conception of evolving lineages did not develop with any degree of sophistication after 1826. By then he had elaborated an invertebrate continuum

Spongilla→*Spongia*→*Cliona*→*Alcyonium*, and implicitly invoked a ‘forking’ lineage by mooted unaltered ‘parents’ and modified offspring. But the branching remained implicit. He never went on to develop it, even though Fleming articulated a branching taxonomy of sponges in 1829 based on Grant’s findings. Fleming realized that

if, in the family constituting the genus *Spongia* of Linnaeus, so well illustrated by the labours of Dr. Grant, we take the common sponge as the type of the spongia of Aristotle, we shall find that, in reference to its skeleton, which is entirely albuminous, it exhibits the greatest simplicity of structure. If we consider this genus as the stem of the family, we shall find a branch issuing forth at each side, the one branch having its albuminous skeleton strengthened by siliceous spicula, forming the genus *Halichondria*, – the other branch with calcareous spicula, forming the genus *Grantia*.

Grant obviously found greater need to establish a serially progressive pedigree in the 1820s and 1830s than a rambling genealogy. Even after Lyell in *Principles* (1832) and William Kirby in his Bridgewater Treatise (1835) attributed a genealogical tree to Lamarck Grant did not develop the image (176). In this he differed from Darwin, who was to realize an “irregularly branched” tree of life early in the 8 Notebook (i.e. in the Summer of 1837) (177). Still, Grant had

fashioned a unique palaeo-environmental transformism, and it remains now for us to examine the materialist social ideologies in Edinburgh in 1826 which could have helped sustain his deistic science.

The Plinian Society: Materialism Is Openly Discussed

In the library of Edinburgh University there is a curious document, the Minute Book of the Plinian Society for the years 1826-41. It was the custom of the Society for the secretary to write an account of each meeting with which was given a clear and fairly detailed summary of any scientific report. The Minute Book reports Darwin's election to the society. He was proposed by several members, among them one W. A. Browne, on November 28, 1826.

Howard E. Gruber, *Darwin on Man* (1974) (178).

Gruber was first to point out the importance of the Plinian minutes, opening up the question of Darwin's exposure to materialist ideologies while at Edinburgh, in particular to the reductionist philosophy of mind expounded by William A. F. Browne (1805-1885). Curiously, Gruber does not seem to know who Browne was. Yet only by understanding his allegiance to George Combe's reformist phrenology can we appreciate the social significance of Browne's materialism at the Plinian or fiercely anti-religious sentiments voiced in the *Phrenological Journal*. And we need to appreciate *that* in order to estimate the ideological support Grant *qua* philosophic deist could have counted on.

Jameson was nominally head of the Plinian Society, which

he had founded 1823. But in fact he told Commissioners looking into the University's affairs in 1826 that, although he approved the proceedings, he actually had no particular control over the members' conduct (179). There is no doubt that Grant's mechanistic physiology and transformism is best viewed against this Plinian backdrop. For one thing he was closely associated with the Plinian Society at this crucial time: its Secretary until 23 May 1826, and a Council member in November, when Browne was one of the five Presidents (180). Grant took part in the animated debates following the more heretical papers (181). It is probably no coincidence that he was advocating transformism at exactly the time that materialist philosophies were hotly debated at the society. A number of Plinian members held flagrantly reductionist views, but in 1826-7 Browne was the most outspoken. He was already a member of the Phrenological Society (elected 1824) and a disciple of George Combe (1788-1858) (182). Browne took his diploma at Edinburgh in 1826, and quickly joined the phrenological elite. In January 1828, at the dinner given by the Phrenological Society in honour of Spurzheim's visit, Browne was toasted by Combe for his success "in bringing before the medical students of Edinburgh the importance of Phrenology as the doctrine of the functions of the brain" (183). He was in demand as a visiting lecturer on phrenology in the early 1830s, while teaching physiology and zoology at the Edinburgh Association (184). In 1834 he was to be appointed medical superintendent at the Montrose Lunatic

Asylum, despite his craniology being urged against him – apparently the first such appointment in Scotland falling to a phrenologist (185). He held enlightened views on the treatment of the insane, and wrote his influential *What Asylums were, are, and ought to be* (1837) “for the specific and avowed purpose of demanding from the public an amelioration of the condition of the insane” (186). His sensitive moral therapy was characteristic of the phrenological alienists (187), but the appeal of the book stretched far beyond phrenological confines to the medical reform movement generally; hence it was praised by the radical Wakley in *The Lancet* (also by this time championing Grant’s case). In *What Asylums were* Browne advocated a material basis for mental alienation, urging that it should be treated as a fault of the brain, not the mind (188). His secularism was again characteristic not only of phrenological alienists but of champions of labour among medical reformers (a group which included Grant). And his “Observations of Religious Fanaticism”, a study of nine cases from his own asylum proving that those canonized by the Church for their hyperactive organ of veneration would today be diagnosed as insane (189), would have been relished by Wakley.

Browne’s philosophy was already strongly reductionist in 1826-7, when he debated the material basis of mind at the Plinian Society. While Grant was still Secretary Browne discussed a topic of perennial interest to alienists, the “Philosophy of Apparitions (25 April 1826), controversy over

which had been stirred up by Samuel Hibbert's book (190). Browne believed that "spectral appearances" were caused by a "superabundant circulation of the blood" to parts of the brain (191). Since phrenologists assumed that an influx of blood resulted from inflammation or nervous irritation (192), a material cause was clearly assignable. Browne's paper evoked "considerable discussion" among the members, and most of those who spoke up put visions down to organic malfunction (193). His subsequent papers exposed his trenchant materialism. Late in 1826 he "refuted" Charles Bell's views on the "Anatomy and Physiology of Expression". Both Grant and Darwin were present (194). Then on 27 March 1827 came the incident noted by Gruber: from the Chair, Browne read a paper "on organization as connected with Life & Mind", which ended with the proposition "That mind as far as one individual sense, & consciousness are concerned, is material" (195). The record of Browne's paper was subsequently neatly stricken from the record, as was the previous week's announcement. On whose orders this was done is not known (perhaps Jameson's – he might have had no control over conduct but he undoubtedly reserved the power of veto*). The paper itself generated a

* Jameson had no love of phrenology and took steps elsewhere to eradicate it. The phrenologists were furious that he denied students interested in craniology access to the skull collection from the Paris catacombs. As keeper of the Museum he had the power to do this, even though the skulls had originally been selected by M. Royer to illustrate organ development (196). Nor was this behaviour exceptional; Chitnis reveals that Jameson exploited the Museum for explicitly Wernerian ends, for example, by limiting access to Hutton's collection (197). Evidently his policy on admission

heated discussion between Grant, Browne, Ainsworth, and W. R. Greg. Greg's own pronouncements confirm the materialist trend at a number of meetings. On 12 December 1826 he read a paper "on the mental and instinctive powers of Brute Creation", which was again the occasion for a discussion involving Grant and Browne. And Grant followed up Greg's paper on instinct on 27 February "in which he attempted to prove that the lower animals possess every, faculty & propensity of the human mind" (200).

So Grant was in the thick of the materialist debates at the Plinian, and clearly the mood of the society on these occasions was congenial to his deistic Lamarckism and biological reductionism. Whether or not Greg and Browne were extremists and unrepresentative of Plinian members as a whole is not the point. The prevalence of materialism among medical graduates is well documented; we get a vivid impression of the strength of the ideology by gauging its effect on those

was formulated for ideological ends. William F. Ainsworth (1807-1896) – another Plinian President – complained to the Royal Commissioners that as a student in Jameson's class in 1827 he too had been refused permission to see some skulls (198); and in his own newly-founded *Edinburgh Journal of Natural and Geographical Science* (1830) attacked Jameson's singular mismanagement of the Wernerian Society and College Museum. Interestingly Ainsworth's grievances now included the misapplication of Grant's invertebrate collection (which he had deposited in the Museum). All students "engaged in immediate scientific pursuits", insisted Ainsworth, should have "unreserved admission" to the Museum, while "the drawers, which imprison the insects, should be thrown open; – [and] the collection of specimens and preparations from the Frith of Forth, presented by Dr. Grant, should be applied to the use for which he intended them ... " (199).

of a different persuasion. I have in mind another of the Society's Presidents in 1826-7, John Coldstream (1806-1863). We have biographical testimony that both before and after his medical education at Edinburgh (1823-7) he showed an unwavering evangelical piety. Before matriculating he wrote for the Leith Juvenile Bible Society. And later in life he devoted himself to medical missionary society work and became an Elder of the Free Church (201). Darwin in 1826 found him "prim, formal, highly religious and most kind-hearted" (202). His evangelical biographers all agreed that he suffered a spiritual crisis immediately on leaving Edinburgh: as his son says, "Some great change had manifestly passed upon his soul" (203). Coldstream's scientific interests were meteorology and invertebrate zoology. Between 1823 and 1829 he communicated papers to the student societies on *Spongia*, *Loligo*, and Firth of Forth zoophytes. He came into contact with Fleming through the Wernerian Society and by 1824 was already sending the Minister notes on *Flustra*, holothuria, *Alcyonium*, *Doris*, and *Balanus* (204). Thus he was working on *precisely* the area which Grant had made his own; and the two were on friendly terms. Grant studied Coldstream's collection and published descriptions of his specimens – for example, the rare *Octopus ventricosus* (205). Probably Coldstream was familiar with Grant's transformism. As one of the Plinian Presidents in 1824 and 1825, and active in its proceedings until 1827, he was closely associated with the materialists (206) (indeed he proposed Browne for the Society). So it is reasonable to hold this group wholly or partly responsible for the "crisis of

his soul's history". By 1825 Coldstream was suffering fears of apostasy. He was troubled by his failure to act more positively in the name of Christ. 1826-7 were years of fervent prayer and nagging doubt, the breakdown finally occurring in Paris (where he had gone to finish his education). The Glaswegian ophthalmologist William Mackenzie met Coldstream in Paris and wrote:

Though a young man, I believe of blameless life, still he was more or less in the dark on the vital question of religion, and was troubled with doubts arising from certain Materialist views, which are, alas! too common among medical students. He spoke to me of his doubts, and manifested anxiety on the subject of religion (207).

In more hospitable theological climes, Coldstream did undergo a spiritual reawakening, and he went on to found and dedicate his life to the Edinburgh Medical Missionary Society.

So the impact of materialism was felt even by those who appeared *prima facie* least susceptible. This raises the question of why the ideology exerted itself so strongly, and to what extent one can explain its prevalence by understanding it as a 'tool' to legitimize specific social claims. Browne's case certainly lends itself to Shapin's mode of sociological analysis. Browne was a leading disciple of the social reformer George Combe and a popular phrenological lecturer to lower and middle-class audiences. As an ideology of self-help and social change, phrenology obviously appealed to the labouring classes, yet it was also attractive to the

Edinburgh manufacturers and merchants, the fast rising, politically-underrepresented group which resented the disproportionate power of the established corporations.

Phrenology was a powerful weapon for social change; it threatened to undermine the ancient authorities of University, Kirk, and Tory-gentry alliance (208). Roger Cooter points out in addition that since evangelical reformers were already targeting asylums, alienists (like Browne) were often reformers before they were phrenologists, indeed that a reformist disposition would have made phrenology uniquely attractive to them (209). However, the extension of Shapin's mode of analysis to transformism presents problems. For one thing, Lamarckism did not flower into a popular movement at this time; thus we cannot apply a prosopographical analysis to determine the sort of *social* work it was expected to do. Only much later, in 1844, when that other phrenologist and friend of George Combe, Robert Chambers, published the popular *Vestiges*, did a theory of providential development acquire a strong social base, at once reformist and fashionable. Yet considered as a rival form of social disobedience, Grant's and Knox's materialist transformism showed intriguing parallels to Browne's and Combe's phrenology. Both were imbued with an Enlightenment faith in social progress; members of both parties campaigned for social and medical change – Browne championed asylum reform and the introduction of moral therapy, Grant fought for institutional and anti-monopolistic medical reform. Thus vociferous Radicals like Wakley applauded both. Phrenologists were enthusiastic

supporters of the new liberal London University where Grant was to teach, and which they portrayed as an antidote to the great bane of society, “the prevalence of aristocratic feeling” (210). Although it was in fact the *Phrenological Journal* which moaned in 1826 that

prejudice and ignorance stand opposed at present to the erection of a great and splendid national university in London on a liberal constitution, and free from the monkish trammels which bind up the human faculties in Cambridge and Oxford

it could easily have been Grant, Wakley, Knox, or Combe (211). Grant was not a craniologist, but he was prepared to defend Gall’s neuroanatomy, and he might have had sympathy for Browne’s trenchant materialism. At the same time, the increasing environmentalism of phrenologists in the later 1820s – what Shapin designates their “‘Lamarckian’ social determinism” (212) – makes comparison with Grant’s evolutionism with its environmental drive even more attractive; while Grant’s organic escalator progressed inexorably towards the human apex, Combe’s phrenologically reformed society was expected through social conditioning to reach an acme of intelligence and spirit. Perhaps it was because he was an academic zoologist that Grant saw Lamarckism fulfill his needs for an anti-Establishment ideology. But though he adopted a rival naturalistic theory, owing more to French deists like Lamarck and Geoffroy, he was still rejecting traditional values in much the same way as the more popular phrenologists.

Notes and References

1. B. Barnes, *T. S. Kuhn and Social Science* (London, Macmillan , 1982) , xi.
2. S. Shapin and A. Thackray, “Prosopography as a Research Tool in History of Science: The British Scientific Community 1700-1900”, *Hist. Sci.*, 12 (1974) 1-28.
3. L. Eiseley, *Darwin's Century. Evolution and the Men who discovered it* (New York, Anchor Books, 1961), 145.
4. S. Shapin, “Phrenological Knowledge and the Social Structure of Early Nineteenth-Century Edinburgh”, *Ann. Sci.*, 32 (1975), 219-43; J. B. Morrell, “Professors Robison and Playfair, and the *Theophobia Gallica*: Natural Philosophy, Religion, and Politics in Edinburgh, 1789-1815”, *Notes and Records of the Royal Society*, 26 (1971), 43-63.
5. J. B. Morrell, “The Leslie Affair: Career, Kirk, and Politics in Edinburgh in 1805”, *Scottish Hist. Rev.*, 54 (1975), 63-82; idem., “Science and Scottish University Reform: Edinburgh in 1826”, *Brit. J. Hist. Sci.*, 6 (1972), 39-56.
6. R. G. A. Dolby, “Reflections on Deviant Science”, in Roy Wallis (ed.), *On the Margins of Science: The Social Construction of Rejected Knowledge*, Sociology Review Monograph 27 (1979), 9-47 (25).
7. H. Lonsdale, *A Sketch of the Life and Writings of Robert Knox the Anatomist* (London, Macmillan, 1870), 402.
8. As a professional group lawyers in later eighteenth century Edinburgh were highly trained and socially well-connected, tied (often by marriage) to gentry more than mercantilists. Street directories in the 1770s listed citizens in the order of advocates, writers to the signet, nobility, gentry, and then the middle classes in an undifferentiated fashion. T. C. Smout, *A History of the Scottish People 1560-1830* (Collins, London, 1969), 373-6.
9. BS, 689; W. Sharpey, Obituary Notice of Dr Robert Edward [sic] Grant”, *Proc. Roy. Soc. Edinburgh*, 8 (1875), 486-90 (490); see also the naval diplomas of Francis and James Grant, and notice of the effects of Lieut. Ludovic Grant (d. 1836): uncatalogued box, UCL.
10. BS, 689.
11. On Grant’s hatred of providentialism see, *Tabular View*

of the Primary Divisions of the Animal Kingdom (London, Walton & Maberly, 1861), 3; E. A. Schafer, “William Sharpey”, *University College Gazette*, 2 (36) (October 1901), 215; and particularly R. J. Godlee, “Thomas Wharton Jones”, *Brit. J. Ophthalmology*, 93 (1921), 145-81. See also my Ch. 5.

12. 1791 according to Lonsdale (op. cit.7), 4; but cf. I. Rae, *Knox the Anatomist* (Edinburgh & London, Oliver & Boyd, 1964), 2; and DSB which give the date as 1793.
13. As such he was never wholly sympathetic to proletarian aims. Knox is often portrayed supping up to nobility. Apologetically Rae, ibid., 44-5, admitted that he disliked “what he called in his superior way the *canaille*, or quoting the earlier Knox, ‘the rascal multitude’”.
14. See particularly Morrell, op. cit. (4), Rae, op. cit. (12), 2-3; and Lonsdale, op. cit. (7), 2-4.
15. W. Steven, *The History of the High School of Edinburgh* (Edinburgh, MacLachlan & Stewart, 1849), Appendix, 136.
16. Ibid, 167; Rae, op. cit. (12), 3; Lonsdale, op. cit. (7), 4; BS, 689.
17. Biographical details are taken from J. A. Symonds (ed.), *Miscellanies by John Addington Symonds M.D.* (London, Macmillan, 1871), and H. Hack Tuke, *Prichard and Symonds in Especial relation to Mental Science* (London, Churchill, 1891), 31. Note however that Symonds was a liberal, and was warned in 1831 after taking up residence in battle-torn Bristol that to vote for reform would ruin his practice. He stuck to his guns and voted against the Tory faction.
18. BS, 690; Grant’s notes on “Dr. Barclay’s [60] Lectures on Comparative Anatomy” of 1821 are bound in his MS “Essays on Medical Subjects”, UCL MS Add 28.
19. BS, 689.
20. J. Struthers, *Historical Sketch of the Edinburgh Anatomical School* (Edinburgh, MacLachlan & Stewart, 1867), 81, also 74-5; BS, 689, 690.
21. A. Fyfe, *Outlines of Comparative Anatomy* (Edinburgh, Black, 1813), Advertisement, v. Fyfe’s *Compendium of Anatomy* had passed through 9 editions by 1826. Grant owned the 6th ed. of Vol. IV, i.e. the *Outlines*.
22. Grant, “An Essay on the Comparative Anatomy of the Brain”, contained in “Essays on Medical Subjects”, UCL MS Add 28. In places he was positively extravagant in his praise, claiming that Fyfe had “done more to ad-

vance [human anatomy] than ever fell to the lot of any other individual" (ff. 16-7). It would be useful to know more about Grant's relationship with Fyfe, who comes across very much as the underdog of his time.

23. BS, 689, 692. They are presumably the four MS lectures bound in the above cited "essays", viz: "An Essay on Gastrilis" (1813-4), "An Essay on the Circulation of the Blood in the Foetus" (1813-4), also his inaugural dissertation, "An Essay on the Morbid Anatomy of the Intestines" (1814-5), and the "Essay" in note 22.
24. BS, 689.
25. Struthers, op. cit. (20), 71. On Gordon's life see D. Ellis, *Memoir of the Life and Writings of John Gordon* (Edinburgh, Constable, 1823).
26. [J. Gordon], "Functions of the Nervous System", *Edinburgh Review*, 24 (1814-5), 439-52 (439).
27. The Wellesley Index lists only two; however internal evidence establishes beyond doubt that "Abernethy on the Vital Principle", *Edinburgh Review*, 23 (1814), 384-98, was Gordon's doing as well.
28. Ibid., 384.
29. Ibid., 387-8.
30. [Gordon], op. cit. (26), 445. Spurzheim anticipated objections touching on the sensitivity of acephali by distinguishing consciousness from "automatic motions", which operate "according to determinate laws without consciousness, reflection, or will": J. G. Spurzheim, *The Physiognomical System of Drs. Gall and Spurzheim* (London, Baldwin, Cradock, & Joy, 2nd ed. 1815), 124.
31. G. N. Cantor, "The Edinburgh Phrenology Debate: 1803-1828", *Ann. Sci.*, 32 (1975), 195-218 (205). A more specialized study of Gall and cerebral localisation is to be found in R. M. Young, *Mind, Brain and Adaptation in the Nineteenth Century* (Oxford, Clarendon Press, 1970).
32. T. Brown, *Observations on the Zoonomia of Erasmus Darwin M.D.* (Edinburgh, Mundell, 1798).
33. Cantor, op. cit. (31), 198.
34. R. E. Grant, *De Circuito Sanguinis in Foetu* (Edinburgh, Ballantyne, 1814), 8; idem., *Tabular View*, op. cit. (11), v. For discussions of Erasmus Darwin see P. J. Bowler, "Evolutionism in the Enlightenment", *Hist. Sci.*, 12 (1974), 159-83; W. F. Bynum, "The Great Chain of Being after Forty Years: An Appraisal", *Hist. Sci.*, 13 (1975),

35. [J. Gordon], “The Doctrines of Gall and Spurzheim”, *Edinburgh Review*, 25 (1815), 227-68 (227); Ellis, op. cit. (25), 37-99, for a partisan view of the affair.
36. [Gordon], *ibid.*, 228.
37. *Ibid.*, 232; cf. Spurzheim, op. cit. (30), 452-61.
38. *Ibid.*, 232.
39. *Ibid.*, 240-5 (243); Spurzheim, op. cit. (30), 487-524, answers the charges of materialism, fatalism, and denial of “moral liberty”.
40. Spurzheim, op. cit. (30), 553. G. N. Cantor, “A Critique of Shapin’s Social Interpretation of the Edinburgh Phrenology Debate”, *Ann. Sci.*, 33 (1975), 245-56 (252).
41. S. Shapin, “The Politics of Observation: Cerebral Anatomy and Social Interests in the Edinburgh Phrenology Disputes”, in Wallis, op. cit. (6), 139-78 (144).
42. Grant, op. cit. (22), ff. 14-5.
43. He owned Spurzheim’s enlarged edition of *Physiognomical System* (1815), and *The Anatomy of the Brain, with a General View of the Nervous System* (London, Highley, 1826: trans. R. Willis).
44. Spurzheim, op. cit. (30), 7-8, 128-32.
45. Grant, op. cit. (22), ff. 24-5.
46. *Ibid.*, ff. 25-6.
47. Lonsdale, op. cit. (7), 400.
48. Shapin, op. cit. (4), 223-6.
49. J. H. Balfour, *Biography of the Late John Coldstream, M.D., F.R.C.P.E, Secretary of the Medical Missionary Society of Edinburgh* (London, Nisbet, 1865), 38-9. [Anonymous], “Obituary. W. A. F. Browne, Dumfries”, *Medical Times and Gazette*, 1 (1885), 364.
50. Almost nothing is known of Addison’s early life, nor of his trip to Paris. He became house-surgeon to the Lock Hospital and eventually assistant physician (1824) and in 1827 Professor of Materia Medica at Guy’s Hospital: Dr Wilks and Dr Daldy, *A Collection of the Published Writings of the Late Thomas Addison, M.D.* (London, New Sydenham Society, 1868) include a woefully-inadequate biography, pp. ix-xvii.

51. C. Limoges, “The Development of the Muséum d’Histoire Naturelle of Paris, c. 1800-1914”, in R. Fox and G. Weisz (eds.), *The Organization of Science and Technology in France 1808-1914* (Cambridge University Press, 1980), 211-40 (222).

52. R. E. Grant, “Catalogue of Robert Grant’s Library”, UCL MS Add. 58, lists two notebooks (now lost) marked “Tour in Germany 1820, 21, 22”.

53. BS, 691.

54. On the political and institutional aspect of French science in this period, and the use of patronage in an increasingly professionalized context, see D. Outram, “Politics and Vocation: French Science, 1793-1830”, *Brit. J. Hist. Sci.*, 13 (1980), 27-43; and R. Fox, “Scientific Enterprise and the Patronage of Research in France 1800-70”, in G. L’E. Turner (ed.), *The Patronage of Science in the Nineteenth Century* (Leiden, Noordhoff, 1976), 9-51. On Cuvier and the loyalty of his minions, see T. Appel, “The Cuvier-Geoffroy debate and the Structure of Nineteenth-Century French Zoology”, Princeton University Ph.D. thesis, 1975, ff. 113-20 and *passim*.

55. R. E. Grant, “Baron Cuvier”, *Foreign Review*, 5 (1830), 342-80 (368, 344). Apparently Cuvier forwarded information to enable Grant to complete the biography: see *British and Foreign Medical Review*, 15 (1843), 24.

56. R. Owen, MS Notebook 5 (1831), BM(NH) entry for Saturday 20 August.

57. Thus the Académie des Sciences retain none of Grant’s letters to Geoffroy (information courtesy of the archivist Pierre Berthon). To my knowledge the only known copy of a letter from Grant is that reproduced by Geoffroy in “Considérations sur des oeufs d’Ornithorinque”, *Annales des Sciences Naturelles*, 18 (1829), 161-4.

58. Geoffroy to Grant, 10 Sept. 1836, published in BS, 691-2.

59. Limoges, op. cit. (51), 221-5.

60. Rae, op. cit. (12), 23.

61. R. Knox, *Great Artists and Great Anatomists: A Biographical and Philosophical Study* (London, van Voorst, 1852), 19, 96-7; idem., *The Races of Men: A Fragment* (London, Renshaw, 1850), 440. For Joseph Pentland’s insider’s view of Cuvier’s laboratory at this time see W. A. S. Sarjeant and J. B. Delair, “An Irish Naturalist in Cuvier’s Laboratory. The Letters of

Joseph Pentland 1820-1832”, *Bull. Brit. Mus. (Nat. Hist), Historical series*, 6 (1980), 245-319. Lyell thought the comparative anatomy displays in the Jardin “very beautiful” and tempting anyone to stay: K. Lyell (ed.), *Life Letters and Journals of Sir Charles Lyell, Bart.* (London, Murray, 1881), i, 60.

62. Knox, *Great Artists*, ibid., 17.
63. Knox, *Races of Men*, op. cit. (61), 440-1.
64. J. A. Symonds, “Ten Years: A Lecture delivered at the Bristol Institution, January 14, 1861, in *Miscellanies* op. cit. (17), 72-91 (80).
65. Appel, op. cit. (54), f. 185.
66. Knox, *Great Artists*, op. cit. (61), 212; in 1827 he proposed the “doctrine of types”: *Races of Men*, op. cit. (61), 442.
67. Geoffroy St. Hilaire, “Recherches sur l’Organisation des Gavials”, *Mémoires du Muséum d’Histoire Naturelle*, 12 (1825), 97-155 (151); Appel, op. cit. (54), 343 et seq.
68. Appel, op. cit. (54), 211; A. Cobban, *A History of Modern France. Volume 2: 1799-1871* (Harmondsworth, Penguin, 1965), 94-106.
69. Knox, *Great Artists*, op. cit. (61), 25.
70. Knox, *Races of Men*, op. cit. (61), 428.
71. Smout, op. cit. (8), 477 comments that they were rich enough to build their own halls.
72. *The London Medical Directory 1847* (London, Churchill, 1847), 63.
73. A. C. Chitnis, “The University of Edinburgh’s Natural History Museum and the Huttonian-Wernerian Debate”, *Ann. Sci.*, 26 (1970), 85-94 (92).
74. J. Duns to R. Owen, 2 Nov. 1858, BM(NH) OC vol. 10, f. 249.
75. J. H. Brooke, “Natural Theology and the Plurality of Worlds: Observations on the Brewster-Whewell Debate”, *Ann. Sci.*, 34 (1977), 221-86 (254); cf. L. E. Page, “John Fleming”, *DSB*.
76. See John Duns’ memoir of Fleming prefixed to Fleming’s *The Lithology of Edinburgh* (Edinburgh, Kennedy, 1859), vii-lx (xxxviii, xl).

77. P. Corsi, “The Importance of French Transformist Ideas for the Second Volume of Lyell’s Principles of Geology”, *Brit. J. Hist. Sci.*, 11 (1978), 221-44 (224).

78. Brooke, op. cit. (75), 231.

79. [D. Brewster], “Vestiges of the Natural History of Creation”, *North British Review*, 3 (1845), 470-515 (472).

80. [D. Brewster], “Whewell’s *Astronomy, and General Physics*”, *Edinburgh Review*, 58 (1833-4), 422-57 (435-6).

81. Ibid., 445-6.

82. Brewster, op. cit. (79), 500-1 (my emphasis).

83. R. E. Grant, “On the Structure and Characters of the Lernaea oblongata, Gr. a New Species from the Arctic Seas”, *Edinburgh Journal of Science*, 7 (1827), 147-55.

84. Thus presenting Grant a platform to expound his own invertebrate classification: [R. E. Grant], “Zoophytology”, *Edinburgh Encyclopaedia*, 18 (2) (1830), 838-46.

85. D. Brewster to Lord Brougham, 24 July 1827, UCL MS College Correspondence 445.

86. J. Barclay, *Introductory Lectures to a Course of Anatomy* (Edinburgh, MacLachlan & Stewart, 1827), 168.

87. Ibid., 140.

88. B. Ballingball, “Memoir of the Life of the Author” in op. cit. (86), vii-xix (xii).

89. J. Barclay, *An Inquiry into the Opinions, Ancient and Modern, concerning Life and Organization* (Edinburgh, Bell & Bradfute, 1822), 522, 526.

90. Ibid., 30, also 36, 126-52.

91. [R. E. Grant], “Observations on the Nature and Importance of Geology”, *ENPJ*, 1 (1826), 293-302 (297). See my Ch. 4.

92. Op. cit. (33).

93. See his 1821 MS notes of Barclay’s course, op. cit. (18).

94. The status of Barclay’s class was obviously important and Grant would afterwards style himself on his papers “formerly Lecturer on Comparative Anatomy”.

95. Struthers, op. cit. (20), 92.

96. Ibid., 82.

97. Symonds, op. cit. (63), 80.

98. J. Fleming, *Philosophical Zoology: or a General View of the Structure, Functions, and Classification of Animals* (Edinburgh, Constable, 1822), i, 14.

99. Grant refers to Fleming casually as “my friend” in “Notice of Two New Species of British Sponges”, *ENPJ*, 2 (1827), 203-4.

100. R. E. Grant, “Remarks on the structure of some Calcareous Sponges”, *ENPJ*, 1 (1826), 166-71.

101. [J. Fleming], “Systems and Methods in Natural History”, *Quarterly Review*, 41 (1829), 302-27 (321); idem., *A History of British Animals* (Edinburgh, Bell & Bradfute, 1828), 518.

102. Fleming, *History*, ibid., 524.

103. [Fleming], “Systems”, op. cit. (101) 320.

104. Fleming, op. cit. (98), ii, 96-70 102-4; cf. idem., “Systems”, op. cit. (101).

105. Cf. the arguments over the complexity of Lamarck’s “machinery” in C. Lyell, *Principles of Geology* (London, Murray, 1832), ii, 13-4; and W. Whewell, *History of the Inductive Sciences* (London, Parker, 1837), iii, 578.

106. [Fleming], “Systems”, op. cit. (101), 320-1.

107. Fleming, op. cit. (98), ii, 137-8.

108. Ibid., i, 121, 310-2; Corsi, op. cit. (77), 222-4.

109. Fleming, op. cit. (98), i, 27.

110. [Fleming], “Systems”, op. cit. (101), 323-4; cf. Grant’s view in my Ch. 4.

111. [Fleming], ibid., 321-2.

112. Brooke, op. cit. (75), 252.

113. J. Fleming, “The Geological Deluge, as interpreted by Baron Cuvier and Professor Buckland, inconsistent with the Testimony of Moses and the Phenomena of Nature”, *EPJ*, 14 (1826), 204-39 (214, 208, 215).

114. Cf. L. E. Page’s comments in “John Fleming”, *DSB*.

115. L. E. Page, “Diluvialism and its Critics in Great Britain

in the Early Nineteenth Century”, in C. J. Schneer (ed.), *Toward a History of Geology* (Cambridge Mass., The MIT Press, 1969), 257-71 (264).

116. R. E. Grant, “Lectures”, *The Lancet* 1 (1833-4), 351.
117. J. Fleming, “Additional Remarks on the Climate of the Arctic Regions, in answer to Mr Conybeare”, *ENPJ*, 8 (1830), 65-74 (68).
118. Wernerian Society Minutes, vol. 1, f. 249, Edinburgh University Library MS Dc.2.55.
119. J. Fleming, “On the Value of the Evidence from the Animal Kingdom, tending to Prove that the Arctic regions formerly enjoyed a milder Climate than at present”, *ENPJ*, 6 (1828), 277-86. Cf. my discussion of Grant below.
120. Fleming, *History*, op. cit. (101), 518.
121. Rae, op. cit. (12), 31-7.
122. Godlee, op. cit. (11).
123. See the title page of R. E. Grant, “Observations on the Structure and the Functions of the Sponge”, *ENPJ*, 2 (1827), 121-41.
124. A subject of no less interest to Jameson, see his “Catalogue of Animals, of the Class Vermes, found in the Frith of Forth, and other parts of Scotland”, *Memoirs of the Wernerian Natural History Society*, 1 (1808-10), 556-65.
125. Knox published 6 articles in the *Memoirs* in 1824-5, including the important multi-part “Observations on the Anatomy of the Duck-billed Animal of New South Wales, the Ornithorhynchus paradoxus of Naturalists”, *Memoirs of the Wernerian Natural History Society*, 5 (1824), 26-41, 144-73.
126. The *Paramelus* (shipped to Jameson, like other Australian specimens, by Sir Thomas Brisbane) was presented to Grant for dissection. Sir A. Grant mentions Brisbane and the colonial suppliers in *The Story of the University of Edinburgh during its first three hundred years* (London, Longman, Green, 1884), i, 376-7.
127. BS, 690; L. Jameson, “Biographical Memoir of the Late Professor Jameson”, *ENPJ*, 57 (1854), 1-49 lists Grant among Jameson’s more distinguished protégés.
128. “Notes on the Geology of Scotland from Jameson’s Lectures on Nat. Hist.”, 2 April 1823: bound in Grant’s

MS “Essays”, op. cit. (18).

129. [R. E. Grant], “On the Egg of the Ornithorhynchus”, *ENPJ*, 8 (1830), 149-51.

130. His new species include *Spongia nivea*, op. cit. (100); *S. cinerea* and *sanguinea*, op. cit. (99); *S. seriata*, “Observations and Experiments on the Structure and Functions of the Sponge”, *ENPJ*, 13 (1825), 94-107; *S. panicea* (note 147 below); and the new genus *Cliona* (note 144 below).

131. Wernerian Society Minutes, op. cit. (118), f. 248; BS, 690.

132. Bowerbank dedicated the first volume of his *Monograph of the British Spongiidae* (London, Ray Society, Vol. 1, 1864) to Grant “for the lucid and able manner in which he led the way”. Grant himself enthusiastically endorsed Bowerbank’s research, e.g. R. E. Grant to G. G. Stokes, 22 July 1861, Royal Society MS Referees Reports, RR.5 26.

133. Bowerbank told Owen that “there is no Scientific Man in London who is better acquainted with [sponges] than he [Grant] is”: 3 May 1863, BM(NH) OC Vol. 5, f. 358.

134. I. B. J. Sollas, “Porifera (Sponges)”, in S. F. Harmer and A. E. Shipley (eds.), *The Cambridge Natural History* (London, Macmillan, 1906), i, Chs 7-9, pp. 165-242 (167).

135. R. B. Freeman, “Notes on Robert E. Grant” UCL 1964 (xerographed typescript), 1.

136. Lamouroux had published *Exposition méthodique des genres de l'ordre des polypiers* (Paris, Agasse, 1821); Schweigger’s paper “Observations on the Anatomy of the Corallina Opuntia, and some other species of corallines”, *ENPJ*, 1 (1826), 220-4, was communicated to Jameson’s journal by Grant and published with his covering notes – he also distributed the offprints (thus Darwin received one).

137. B. Montagu, “An Essay on Sponges, with Descriptions of all the Species that have been discovered on the Coast of Great Britain”, *Memoirs of the Wernerian Natural History Society*, 2 (1811-16), 67-122 (71).

138. J. E. Gray, “On the Situation and Rank of Sponges in the Scale of Nature, and on their internal Structure”, *Zoological Journal*, 1 (1824), 46-52. T. Bell, “Remarks on the Animal Nature of Sponges”, *ibid.*, 202-4 (204).

139. Bell, *ibid.*, 203; R. E. Grant, “Observations and Experiments on the Structure and Functions of the

Sponge”, *EPJ*, 13 (1825), 333-46.

140. In particular, many of his papers were translated into French (and German), e.g. *Annales des Sciences Naturelles* 11 (1827), 150-210, and his work was followed up by Dujardin, *Comptes Rendus Académie des Sciences*, 7 (1838), 556-67, 617-9.

141. R. E. Grant, “On the Structure and Nature of the Spongilla friabilis”, *EPJ*, 14 (1826), 270-84 (270).

142. Ibid., 283-4.

143. Wernerian Society Minutes, op. cit. (118), f. 261.

144. R. E. Grant, “Notice of a New Zoophyte (*Cliona celata*, Or.) from the Frith of Forth”, *ENPJ*, 1 (1826), 78-81 (79-80).

145. Ibid., 81.

146. R. E. Grant, op. cit. (141), 283.

147. R. E. Grant, “Observations on the Structure of some Silicious Sponges”, *ENPJ*, 1 (1826), 341-51 (350).

148. Grant, op. cit. (123), 138.

149. [J. Playfair], “Geographie Mineralogique des Environs de Paris”, *Edinburgh Review*, 20 (1812), 369-86 (382).

150. R. Porter, *The Making of Geology: Earth Sciences in Britain 1660-1815* (Cambridge University Press, 1980), 145-56; M. J. S. Rudwick, “The Foundation of the Geological Society of London”, *Brit. J. Hist. Sci.*, 1 (1963), 325-55. On the mineralogical-economic interests of the founders of the GS see P. J. Weindling, “Geological controversy and its historiography: the prehistory of the Geological Society of London” in L. J. Jordanova and R. S. Porter, *Images of the Earth: Essays in the History of the Environmental Sciences* BJHS Monograph 1 (1979), 249-71. Also on the composition changes R. Loudon, “Ideas and Organizations in British Geology: A Case study in Institutional History”, *Isis*, 68 (1977), 527-38.

151. Porter, *ibid.*, 152.

152. BS, 690.

153. Hutton’s deism is discussed in D. R. Dean, “James Hutton on Religion and Geology: the Unpublished Preface to the *Theory of the Earth* (1788), *Ann. Sci.*, 32 (1975), 187-93. Playfair did however give Huttonianism a providential gloss.

154. Morrell, op. cit. (4), 52-9.

155. [J. Playfair], “Cuvier on the Theory of the Earth”, *Edinburgh Review*, 22 (1813-4), 454-75.

156. Fleming, op. cit. (113,119).

157. G. Cuvier, *Essay on the Theory of the Earth* (ed. R. Jameson: Edinburgh, Blackwood, 1813).

158. J. H. Ashworth, “Charles Darwin as a Student in Edinburgh, 1825-1827”, *Proc. Roy. Soc. Edinburgh*, 55 (1935), 97-113 (100).

159. E. A. Crouch, *An Illustrated Introduction to Lamarck's Conchology contained in his Histoire Naturelle des Animaux sans Vertèbres* (London, Longman, 1827); C. Dubois, *An Epitome of Lamarck's Testacea: being a free translation of that part of his work, De l'Histoire naturelle des animaux sans vertebres* (London, Longman, 1824).

160. E. J. Macculloch, “Fossil Fishes”, *Edinburgh Review*, 37 (1822), 47-60.

161. E. J. Macculloch, “French Geology of Scotland”, *Edinburgh Review*, 38 (1823), 413-37 (415).

162. M. J. S. Hodge, “Lamarck's Science of Living Bodies”, *Brit. J. Hist. Sci.*, 5 (1971), 323-52; R. W. Burkhardt, *The Spirit of System: Lamarck and Evolutionary Biology* (Cambridge, Ma. Harvard University Press, 1977); R. J. Richards, “The Emergence of Evolutionary Biology in the Early Nineteenth Century”, *Brit. J. Hist. Sci.*, 15 (1982), 241-80.

163. Lacépède in his five volume *Histoire Naturelle des Poissons* (Paris, Plassan, 1800), tom. ii: xxxiv-v, lxi, xlvi-lx, conceded that species could be extinguished either by geological revolutions or organic dissipation, and that a metamorphosis might accompany environmental changes. He instanced the transformation of fossil fish into recent forms.

164. Cuvier, op. cit. (157), 114-5.

165. Grant owned the 4th ed. (1822).

166. G. B. Greenough, *A Critical Examination of the First Principles of Geology in a Series of Essays* (London, Strahan & Spottiswoode, 1819), 281-2.

167. [Grant], op. cit. (89), 296.

168. Richards, op. cit. (162), 248.

169. Fleming, op. cit. (98), i, 27; Geoffroy St. Hilaire, “Rapport fait à l’Académie Royale des Sciences sur un Mémoire de M. Roulin”, *Mémoires du Muséum d’Histoire Naturelle*, 17 (1828), 201-29; M. Roulin, “Inquiries respecting certain Changes observed to have taken place in Domestic Animals, transported from the Old to the New Continent”, *ENPJ*, 7 (1829), 326-38.

170. [Grant], op. cit. (89), 298; R. W. Burkhardt, “The Inspiration of Lamarck’s Belief in Evolution”, *J. Hist. Biol.*, 5 (1972) 413-38.

171. [Grant], op. cit. (89), 299; cf. Fleming, op. cit. (98), ii, 88-96.

172. Cf. his 1853 “Palaeozoology” lectures discussed in my Ch. 8, which also deal with climatic zoning as the central heat diminishes.

173. P. Lawrence, “Heaven and Earth – the Relation of the Nebular Hypothesis to Geology”, in W. Yourgrau and A. D. Breck *Cosmology, History, and Theology* (New York, Plenum Press, 1977), 253-81 (259-66); idem., “Charles Lyell versus the Theory of Central Heat: A Reappraisal of Lyell’s Place in the History of Geology”, *J. Hist. Biol.*, 11 (1978), 101-28 (103-6).

174. [R. E. Grant], “Of the Changes which Life has experienced on the Globe”, *ENPJ*, 3 (1827), 298-301 (299, 301).

175. Ibid., 300, 301.

176. Fleming, op. cit. (101), 321-2; Lyell, op. cit. (105), ii, 10; W. Kirby, *On the Power Wisdom and Goodness of God as Manifested in the Creation of Animals and in their History Habits and Instincts* (London, Pickering, 1835), i: xxviii.

177. Sir G. de Beer, “Darwin’s Notebooks on Transmutation of Species”, *Bull. Brit. Mus. (Nat. Hist.) Historical Series*, 2 (1959-63), 43-4.

178. H. E. Gruber, *Darwin on Man: A Psychological Study of Scientific Creativity* (New York, Dutton, 1974), 39.

179. *Evidence, Oral and Documentary, taken and received by the Commissioners appointed by His Majesty George IV July 23 1826* (London, 1837: 4 vols.), i, 145-6 (12 October 1826).

180. Plinian Minutes MS, Vol.1 1826-28, f. 37, Edinburgh University Library, Dc.2.53.

181. Ibid., f. 57.

182. *Phrenological Journal*, 1 (1823-4), 489-90. Browne was elected to the Plinian on 10 May 1826, having been proposed by Ainsworth and Coldstream.

183. *Phrenological Journal*, 5 (1829), 141-2.

184. Ibid., 8 (1834), 571-2.

185. Ibid., 662-3. Andrew Combe had sought the post, but withdrew after learning of Browne's interest and "cordially supported the pretensions of his young friend": G. Combe, *The Life and Correspondence of Andrew Combe, M.D.* (Edinburgh, Maclachan & Stewart, 1850), 228-9.

186. W. A. F. Browne, *What Asylums Were, Are, and Ought to be* (Edinburgh, Black, 1837), 1.

187. R. J. Cooter, "Phrenology and British Alienists, c.1825-1845", *Medical History*, 20 (1976), 1-21, 135-51. This, and W. F. Bynum's paper on "Rationales for Therapy in British Psychiatry, 1780-1835", which should also be consulted, are reprinted in A. Scull (ed.). *Madhouses, Mad-Doctors, and Madmen: The Social History of Psychiatry in the Victorian Era* (London, Athlone Press, 1981).

188. Browne, op. cit. (186), 4.

189. W. A. F. Browne, "Observations on Religious Fanaticism", *Phrenological Journal*, 9 (1836), 288-302, 532-45, 577-603.

190. Great interest was shown in this book, and not only by alienists and phrenologists; see, in addition to the references below, *ENPJ*, 10 (1824), 378; and Brewster's speculations in *Quarterly Review*, 48 (1832), 287-320. Another Edinburgh student who wrote on apparitions was Symonds, *Miscellanies*, op. cit. (17), 209-64. He too opted for a reductionist 'derangement' theory, although allowing some leeway for deistic control.

191. Plinian Minutes MS, op. cit. (180), ff. 11-12.

192. A phrenological analysis of apparitions was given in *Phrenological Journal*, 1 (1824), 541-55.

193. Plinian Minutes MS, op. cit. (180), f. 12.

194. Ibid., f. 34.

195. Ibid., f. 57.

196. "Phrenology and Professor Jameson", *Phrenological Journal*, 1 (1824), 55-8.

197. Chitnis, op. cit. (73), 90-3.

198. *Evidence*, op. cit. (158), 632.

199. “On the Present State of Science in Great Britain. No. 1. Edinburgh College Museum”, *Edinburgh Journal of Natural and Geographical Science*, 1 (1830), 269-77 (275).

200. Plinian Minutes MS, op. cit. (180), f. 51.

201. Balfour, op. cit. (49); [J. P. Coldstream], *Sketch of the Life of John Coldstream. M.D., F.R.C.P.E. The Founder of the Edinburgh Medical Missionary Society* (Edinburgh, Maclare & Macniven, 1877).

202. N. Barlow (ed.), *The Autobiography of Charles Darwin* (New York, Norton, 1958), 48.

203. [Coldstream], op. cit. (201), 10-11.

204. Balfour, op. cit. (49), 8.

205. R. E. Grant, “On the Structure and Characters of the Octopus ventricosus, Gr. (*Sepia octopodia*, Pent.), a rare species of Octopus from the Firth of Forth”, *ENPJ*, 2 (1827), 309-17.

206. Ainsworth, Grant, and Browne are all mentioned in Balfour’s biography, op. cit. (49), 7.

207. Balfour, *ibid.*, 39.

208. Shapin, op. cit. (4,41); Cantor, op. cit. (31).

209. Cooter, op. cit. (187), 11-12.

210. *Phrenological Journal*, 2 (1825), 432-49.

211. Grant talked of the “monastic ignorance which has so long degraded the Universities of England”: Grant to L. Horner, 5 Nov., 1830, UCL College Corres. P 130. Wakley spoke of the “pestilential vapours” engulfing the ancient universities: *The Lancet* 2 (1830-1), 689-90.

212. S. Shapin, “Homo Phrenologicus: Anthropological Perspectives on an Historical Problem” in B. Barnes and S. Shapin (eds.), *Natural Order: Historical Studies of Scientific Culture* (Beverly Hills/London, Sage, 1979), 41-67 (60).

Chapter 3

Grant's Place In London: Laissez faire, Medical Reform, and the Interests of the Zoological Community

In the last chapter we set Grant's naturalistic Lamarckism into a Presbyterian context, discussing Evangelical notions of Divine Resourcefulness, and offering tentative explanations of Grant's patronage by some of Scotland's fiercest anti-mechanists. The Whig-reformist ideology of many of the affluent professional classes, the social reformism of student phrenologists, and the prevalent materialism of medical men, all helped sustain Grant's Lamarckism and Knox's philosophical anatomy. The evidence of the student societies, extra-mural classes, and journals (1), suggests that Parisian-inspired theories, blending Lamarckian and Geoffroyan elements, and exploiting contemporary Wernerian geology and palaeoclimatology, were discussed more openly than historians have realized.

In the present chapter and chapter five, I want to interrelate a number of contextual themes to highlight the contrasting situation in London. We will see how, after a promising start, Grant's fortunes declined, partly as a result of the University's laissez faire financial arrangements and his inability to generate sufficient income from his lectures. We must assess his science not merely from the

standpoint of the Utilitarian (and to Tories Godless) University, but from the perspective of zoologists in the metropolis, many of whom in this ‘careerist’ (2) pre-professional stage were full-time salaried government staff. We will see that this conservative bureaucracy was unreceptive to his Lamarckian zoology, causing him to lose what institutional footholds he had managed to gain, and with them access to resources. Since he possessed no licence to practise in London he was denied a living from medicine, and joined with radicals like Wakley in demanding the abolition of chartered monopoly. Hence the juxtaposed issues of the title – individualism, reform, and zoological interests – were inextricably bound in Grant’s case. Understanding Grant’s role as a transformist *and* reformer will help us to appreciate the reception accorded his deistic science by radical secularists. Thus to appreciate Grant’s predicament we must take into account the social undercurrents in the years embracing the revolutionary crises of 1831-2 and Chartist demands for political reform – not dismiss them as irrelevant contextual factors, but recognize them as forming a constitutive element in the development of science. Thus we should welcome the current analyses of social historians as providing a crucial explanatory tool with which to reassess the failure of Lamarckism to transfer successfully to the capital.

The London University

Knowing Grant’s Parisian transformist leanings, we might

inquire how he managed to take the Chair of Zoology at the new University. I think that a convincing explanation must embrace a number of factors: the progressive political and social ideology of the founders and stockholders; the Edinburgh-Whig network which worked to his advantage; and even, ironically, the financial unattractiveness of the Chair, which dissuaded ‘established’ applicants. Whatever Grant lacked in experience (two of his rivals were well known teachers), he made up for in youth and potential – two qualities the Council were looking for. He was also at this time publishing prolifically. The fact that his rivals were known to, or part of, the London zoological community, whereas the ‘outsider’ Grant came with impeccable Scottish credentials, suggests at the outset a certain political distancing of the Zoological Department from the conservative community – Grant reflecting more the progressive ideals of many of the founders.

The purpose of the founders in developing a school in London is well known: to serve the increasingly under-represented yet politically powerful merchant population of the capital, to diffuse utilitarian philosophy, and to cater to religious groups – Dissenters, Presbyterians, Catholics, Jews, etc. – traditionally excluded from the Established Universities. The coalition of reform and dissenting groups reflected a precarious convergence of interest. The various networks were often interlocked through personal contact:

Brougham rallied the Whig lobby, telling the Duke of Norfolk (who spoke for the unemancipated Catholics) that the dissenters provided a “great bulwark” against the High Churchmen; and against these religious groups he ranged a powerful body of utilitarians (including James Mill, George Grote, and Henry Warburton), and ‘mechanics’ representatives like George Birkbeck – while radical negro emancipationists like Warburton provided contact with the Evangelicals (3). With factions from unestablished religious groups to ‘infidel’ radicals (coordinated as usual by Francis Place) held in uneasy alliance, it was expedient to keep the institution avowedly secular (i.e. ostensibly neutral) – to the disgust of Tory Anglicans (who promptly set about founding a rival Establishment school in the Strand). But secularism anyway appealed to extreme radicals. As Wakley said with characteristic bluntness, the scheme “afforded not a single compliance with the demands of the ‘Church and State’ bigots of the day”. The new chairs of science “were to be unpolluted by those pestilential vapours which had ever surrounded a certain class of professors in the ‘ancient’ Universities of Oxford and Cambridge” (4).

The second point to note, particularly as it affected the professors, was the financing of the school. In keeping with the individualism of the age and “rage for Joint Stock Companies” (5), the capital of £150,000 was to be raised by selling £66 shares paying a dividend of 6 per cent. To keep out the jobbers, Brougham tried to restrict shares to those

with an interest in the institution. One way was to permit each proprietor to present one student for each share owned. Unfortunately, the whole scheme fell into immediate financial difficulty, and although the money was raised, it never did pay a good return. Moreover the very idea of a joint stock medical school was obnoxious to conservative journals like the new *Medical Gazette*, which believed it to be the “grossest perversion of the desires of the more sober-minded members of the community” (6). Accordingly it lay the fault for the turmoils in the early days at the feet of the governing body “made up almost entirely of lawyers and merchants”, the “class of men” least qualified “by the nature of their pursuits and occupations to regulate the business of a medical school” (7). Never sympathetic, the *Gazette* became positively antagonized by support given the school by radicals like Wakley in the rival *Lancet*.

The point about these joint stock arrangements is that investors expected a return for their outlay. Thus the founders modified the Edinburgh professorial fees system. The new professors were only to be provided “guarantee money” (£300 p.a.) for a tiding-over period of three years (1828-31), after which they were to become self-reliant on fees. Now, the Edinburgh laissez faire system (as Morrell calls it) had some drawbacks. It could induce a ‘protectionist’ mentality among professors; since their livelihood depended on sustaining a critical number of auditors, competition was not

tolerated, nor the creation of new chairs whose subject threatened to impinge on their own. Thus, though the University had had a Regius Chair of Natural History since 1767, the Senatus quashed proposals by the Town Council for the creation of a Chair of Comparative Anatomy and Veterinary Science in 1816 – even though the liberal Duncan protested that “such a Chair would be no prejudice to any existing Professorship, and would be highly advantageous and creditable to the University” (8). (As a result London was the first British University with a Chair of Comparative Anatomy.) The second drawback was that research would suffer if teachers were forced to become showmen to attract fee-paying students. Actually this seemed less of a drawback to capitalist proprietors in London, some of whom viewed the new school more like a teaching factory – an institutional workshop where wealth was generated by churning out the greatest number of trained students. Authorship and research for originators like Thomas Campbell were therefore an unnecessary luxury, incidental to the main purpose. Because of this, conservative romantics like Coleridge, whose ideal was a leisured gentlemanly education to build an elite scientific clerisy, were to dismiss this threateningly bourgeois “lecture bazaar” out of hand (9). But the main point of departure from the Edinburgh system, one which penalized all the professors, but particularly the poorly paid, was that *they* had to generate enough income to provide a return on the investors’ capital. In Grant’s cases any earnings over £100 p.a. (the operative is ‘any’: some years he

did not reach £100!) had to be shared equally with the proprietors (10). Thus in an average year in the 1830s when Grant earned £117 from fees, £8. 10s went to pay the dividend.

The mixed blessings of such an arrangement were already apparent when the Council came to fill the teaching posts. Established savants saw no financial inducement; John Herschel and Charles Babbage turned down the mathematics chair, and although the Glasgow botanist William Hooker refused the botany chair because of the lack of a garden, even his replacement, the young assistant secretary to the Horticultural Society, John Lindley (1790-1865), at first declined the offer on financial grounds. Charles Lyell incisively summed up the situation in a letter to Fleming:

Young Lindley ... told me yesterday that even he is now no longer a candidate; thinking, that as there will be no compulsory attendance for medical students, such a class will be precarious and a failure, and that the University do not guarantee such a minimum as can warrant a man, who has anything certain to give up, in venturing. I fear they have made this mistake, that they have not determined to bribe able men for the chairs that can scarcely be profitable from the classes; for Babbage viewed it in this light when offered the mathematical chair. ‘What they will secure to me,’ he said, ‘is no more than I could make in the same number of hours by authorship, and get more fame. They have no dignity to confer as yet, they have their reputation to make. I have not. If, as they admit, they wish to get some from me, why they ought to buy it, and pay for it’ (11)

Initially this financial bar operated in Grant’s favour, insofar as competition was naturally not great. Only three rival candidates presented themselves (12), and their

credentials were less impressive to the Edinburgh Whig network within the school than the zoological community without. The young missionary in the West Indies Rev. Lansdown Guilding (?1797-1831), applying through the Sowerby family in Regent's Street, would have made an incongruous choice. London zoologists might have been in his debt, as the president of the Zoological Club said in 1826, for labouring unaided in St. Vincent to capture "some new subject" (13), but his dual calling might well have disqualified him. When John Harwood applied he had only recently been appointed Professor of Natural History in the Royal Institution (1826-30), which did not augur well for his staying power (indeed he quit the RI in 1830 after taking a residence out of town (14)). Harwood, although an FRS, seems to have been preeminently a popular teacher; in 1826 he was lecturing on mammals and birds to fashionable audiences at the RI and London Institution (15), and delivering juvenile courses, all of which paid more handsomely than was likely at Gower Street (16). The celebrated anatomist and founder of the Blenheim Street Museum Joshua Brookes (1761-1833) would have posed the most serious challenge, but for his age and health. Brookes had ploughed £30,000 of his own money into stocking his museum, and been elected FRS for his pioneering preservational techniques. It was said that in forty years five thousand students had passed through his hands (17). He was lecturing on the comparative anatomy of birds at the new Zoological Society in 1827 (18), and acting as Chairman of

the Zoological Club in 1827-8 (19). But at 65 he was past his prime; ill-health had forced him to cease teaching at Blenheim Street in 1826, when he began disposing of his museum. This, “the most elegant, extensive, and celebrated in Europe, being the production of individual talent and exertion, regardless of trouble and expense” (20), was offered to Birkbeck for the Gower Street school; and many of Brooke’s specimens were indeed purchased by Grant for his new museum.

Grant’s youth, productivity, progressive affiliations, and impeccable Scottish references (from Brewster, Barclay, Fleming, and Jameson), made him an attractive choice. Moreover the Edinburgh connections of the promoters and choice of Leonard Horner as first Warden worked to Grant’s advantage. In July 1827 Horner consulted with the Edinburgh surgeon John Thomson on Grant’s “attainments, power of teaching, manners, languages, & moral character” (21). Thomson affirmed that, while he had not spoken to Grant for some years, “I know him to be a man of great modesty and learning who has zealously devoted a large portion of his time to zoological pursuits. I know also that he is much esteemed by those who enjoy his acquaintance” (22). And Brewster congratulated Brougham when the choice was made.

Another factor related to the financial exigencies of the medical school benefited Grant. He applied in May 1827 for the Chair of Zoology and Comparative Anatomy, but the Halle anatomist J. F. Meckel also made known his interest in the ap-

pointment. Aware of Meckel's prestige, the Council summarily appointed him to the Chair of Morbid and Comparative Anatomy, giving Grant the Chair of Zoology. At the outset Meckel demanded £1000 p.a., partly as an annuity on his museum. When he made still more exorbitant demands, the Council dropped him and reallocated Comparative Anatomy to Grant (23). This was fortunate as it turned out, because Grant could not have survived on fees from zoology classes alone (a subject listed as an "ornamental accomplishment" in the first *Statement by the Council* in 1827 (24)).

This will be apparent from Tables I and II, listing attendance figures and fees. Table I shows a breakdown between Comparative Anatomy and Zoology in terms of class size and takings from 1828-33 (25).

YEAR	No. Students (CA)	Fees	No. Students (Z)	Fees
1828-9	3 (26)		?5	(total £6. 10s)
1829-30	15	£30	?17	?
1830-1	10	£26	17	£36
1831-2	18	£56	15	£42
1832-3	17	£37	18	£53

Table I

With the reshuffle following the Meckel decision Grant effectively commanded two chairs; he delivered Comparative Anatomy lectures from October until January, when he lectured on Zoology until April or May. Returns however were still

meagre, and in the year beginning autumn 1829 (27) he initiated a third (summer) course, the contents of which however changed reflecting the contingent needs of the struggling new school, as we will shortly see.

Initial Difficulties at the University 1828-31

When I gave up my professional prospects and pursuits at Edinburgh to devote myself, in the University of London, entirely to the cultivation and advancement of the vast departments of science almost neglected in Britain, I confessedly anticipated every encouragement and assistance from the learned Members of our Council, and hitherto I have not been disappointed.

In my endeavours to lay before our students attractive and philosophic views of the subjects entrusted to me, I have had some difficulties to contend with from our very limited means of illustration, from our want of suitable accommodation for specimens, from our want of dissecting rooms, assistants, and every convenience for prosecuting these studies

The proposal you mentioned to me today of introducing a separate lecturer on the structure of the Cow, the Horse, the Sheep and other quadrupeds I not only consider as uncalled for in the present state of the University where the structure of every class of animals is repeatedly and deliberately examined in the course of the session, but I consider it a measure deeply injurious to my departments as tending to connect in the minds of our University students low and vulgar associations with the sciences of Comparative Anatomy and Zoology.

Grant to the Warden Leonard Horner, 16 November 1830
(28).

Inevitably, any, assessment of Grant's position during these years based on the extant correspondence to the Warden or Secretary is bound to be one-sided. Grant's letters preserved at University College London (the only single large

collection) are almost totally bureaucratic in nature: concerning debts to be settled, dates fixed, specimens donated, and so on. Yet, even bearing this in mind, we can learn a surprising amount from this material. As an example of the sort of information they yield, we might examine Grant's worried reply to Horner reproduced above. By November 1830 the medical school was in turmoil. Its finances were in an atrocious state, the doyen of anatomy Charles Bell was contemplating resigning (again), and student militants were demanding the sacking of the incompetent G. S. Pattison. In this deplorable state of affairs, Grant was already thinking in terms of his *sacrifice* – of giving up his “professional prospects” at Edinburgh to devote his life to an unprotected subject at a struggling school. This striking self-perception, reinforced by his refusal to kow-tow to the medical monopolists and thus failure to obtain a licence to practise, paved the way for a kind of siege-state psychology – one persisting throughout the radical thirties and hungry forties and which encouraged his alliance with extreme radicals. It still largely informed his self-image in the “Biographical Sketch”, where it receives prominent expression through Wakley’s acerbic prose. From the outset at the University Grant was hampered by an appalling lack of illustrative material, specimens, class books, etc. (a fact intimately bound up with the school’s financial difficulties). He was caught, in fact, on the horns of a classic declinist dilemma: he stoutly opposed any appointment (like that of an encroaching popular lecturer) which

threatened his livelihood, and yet by refusing to vulgarize and insisting that zoology should be taught on strictly philosophic lines he was cutting his own financial lifeline.

Understanding the school's financial problems and their social effects, we can appreciate the tone of the letters. And having deciphered them, we will be in a better position to interpret the roots of Grant's social predicament – his own teetering finances, declining output, radical alliances, and personal frustrations which spilled over into his cosmological speculations: all subjects which must be comprehended for any rounded attack on the problem of the social/institutional response to his hardened Lamarckism.

The topics surfacing most frequently in this extant correspondence fall under the following headings:

Lectures, Fees, and Students

The Medical School in effect owed a dual allegiance: to the proprietors, who expected a return on their capital, and to the professors like Grant who, while trying to make the subject attractive, first and foremost insisted on an uncompromising philosophic approach. So the declared aims of proprietors and professors were often at odds. Even when finances and morale were at their lowest ebb, in 1830-1, and the Council tried to boost the intake by lowering degree

requirements, Grant still insisted on standards before profit. Cheapening degrees to raise fortunes was a self-defeating policy. As he told Horner:

The cheapness and facility with which our highest honours are to be obtained in every department of the University may, I have no doubt, allure students to take these Degrees, and may improve the condition of those Professors whose departments are thus selected for patronage, but I cannot help thinking that they are calculated to sink our titles and dignities and our vaunting Establishment into contempt with all sensible and reflecting men.

The limited plan of education in the Physical Sciences in our new University for the rising youth of this country is one which I trust they will despise as calculated to perpetuate that monastic ignorance which has so long degraded the Universities of England in the eyes of the more liberal and enlightened countries of Europe (29).

Grant began in 1828-9 by delivering a single course, embracing comparative anatomy and zoology, but with a turnout so low (only three pupils registered for the former (30)) he instructed the bursar to allow students to purchase tickets for one half of the course only (31). The next year he separated the course into two, and because of the lack of illustrative material and desperate need to attract students dropped the fees to £2 for proprietors' nominees and £2.10s for the rest (32). As late as 1830 the class size was still too small to warrant examinations in January and they were held over to the end of the session (33). In April 1829 he proposed to deliver a short summer course on zoology, presumably in an effort to recoup, telling Horner:

Professor Jameson's summer course is always numerously attended at Edinburgh, and probably some of our students may also have leisure and inclination

during the summer months for so interesting a branch of Natural History as Zoology (34).

Although figures are not available, it was presumably not well attended. The following year (1830) he changed its format, delivering summer lectures on “the *organization and history* of the animal kingdom” (35), embracing fossil and recent species. When he opted for a “Fossil Zoology” summer course I do not know, although it was either in 1831 or 1832 (see below). In 1831 he raised his fees again, ostensibly because the museum facilities had improved, although it is difficult to believe that the loss of his “guarantee money” did not influence his decision (36). So long as he was guaranteed £300 p.a. student numbers did not matter; indeed he could afford to plough money back into the course, and offer to institute a Gold Medal at his own expense for his best student (37) (the first being awarded to one of the leaders of student unrest during the Pattison affair, Nathaniel Eis dell (38)). But reliant solely on fees, his finances became increasingly precarious; and his difficulty could only have been obviated either by the Council’s decision to protect vulnerable subjects (an action at odds with the laissez faire interests of the promoters), or by making comparative anatomy compulsory for MD students (which was unlikely so long as the corporations did not include it in the licencing examination).

The Museum

His other recurrent complaint concerned the impoverished state of his museum, and his efforts to stock it. To found a museum unaided and prepare the dissections anew for three academic courses proved inevitably time-consuming. From Edinburgh he had brought about 100 invertebrate specimens, together with microscopes, dissecting instruments and books. In London he had to supply and pay for his own dissection material, but urged that the Council should cover the cost of the permanent (museum) specimens (39). Indeed in June 1828, when optimism still ran high on all sides, the Council *did* provide him with “ample funds” with which to make purchases. Grant shared the founders’ faith: “I have no doubt,” he informed Horner, “that when the existence of the Museum of the University of London, shall be more generally known, we will be inundated with donations from all quarters at home, and from our scientific countrymen in the most distant colonies” (40). There was even talk of hiring a Keeper to cope with the influx; and the Curator of Jameson’s Museum, William Macgillivray (1769-1852), informed Grant that he was willing to take the job for less than the £90 p.a. he was paid at Edinburgh (41). (Retrenchment presumably put paid to the idea of creating this post; in 1832 Grant had to content himself with a boy to help out in the museum (42).) Receipts show that by November 1828 Grant had already spent £241 on specimens. This evidently did not stretch far, because he was still complaining of the “poverty of the Zoological Museum”

(43), and we have seen that he was forced to lower his fees as a consequence. He wrote in alarmist tones in December that he did not possess a single specimen to illustrate the next two classes, birds and reptiles (44). By 1830 students were protesting to the Council about the lack of library books, and Grant was arguing that the state of the museum was more likely to “repel” than attract auditors (45). It did not help that in 1830 a “convocation of politic *mice* and *rats*” forced entry into the museum and devoured the stuffed ducks and dried preparations: “I hope that the language of the rats and mice will have more influence than mine in directing some kind regards to the preservation of the zootomical specimens”, he told Horner the following day (46). Optimism and affluence gave way to cut-backs and recrimination as the school’s financial affairs began attracting criticism, and Grant’s letters became increasingly carping in tone.

So many of the letters to Horner, and after his resignation in 1831 to the Secretary Thomas Coates, are pleas for more space, more specimens, and more money. Grant did make a number of significant purchases: from 1828-30 he bid against Buckland, Clift, and Mantell (representing their respective museums at Oxford, Lincoln’s Inn, and Lewes) at Joshua Brookes’ sale. Unfortunately, as Mantell found, the considerable opposition pushed up prices (47). Grant reported back to Horner that “the specimens in general are selling at a higher price than I could have anticipated” (48). But he did make number of purchases (49), and when the sale finished

the remaining stuffed hippo was presented to the University (50). Moreover there were donations to help the infant museum onto its feet: J. E. Gray at the British Museum and N. A. Vigors at the Zoological Society were helpful in this respect (51). Even Lady Raffles presented, through Vigors, skins brought back by Sir Stamford Raffles from Sumatra (52). Thus by the early 1830s Grant had put together a teaching collection sufficient to accompany his lectures; and by the early 1840s this was augmented by shipments from graduates residing in the colonies, particularly from Edmund Hobson in Van Dieman's Land (53).

Continuing Disturbances: The Effects of the Pattison Affair

Even the arrangements of Grant's courses cannot be totally divorced from the disruptions within the University. He was Secretary of the Medical Faculty from May 1831; by then Charles Bell had resigned and troubles involving Granville Pattison were brewing. Bell, in open conflict with the Council and fearing that the school was "going fast to the dogs", had walked out in December 1830 midway through his introductory lectures on "design" (54). Not all were sad at losing the most celebrated anatomist in London; the outspoken secularist Wakley had seen his academic excellence "obscured by an unsightly, sickening affectation and mannerism" (55). Nonetheless his resignation severely damaged the school's credibility. The next few months proved critical; the school

was eating up its capital, enrollment figures plummeted, and it faced financial ruin. Then the injurious dispute over Pattison's competence publicly broke in an air of spectacle and scandal. Politicking became rife as old scores were settled, and personal enmities clouded what should have been a clear-cut issue: the anatomy professor's competence to teach. Bell's departure had partly been prompted by his contempt for Pattison. Students even testified that the anatomy Demonstrator J. R. Bennett was more proficient than his superior. But the autocratic and disliked Warden Leonard Horner lined up with Bell and so doing alienated a number of professors who sided with Pattison (56). The students led by Grant's medallist Eisdell demanded Pattison's dismissal and nearly rioted. They slated the conniving "parcel of potters and haberdashers" (i.e. Council) and horrified the conservative *Medical Gazette* by mixing "political feeling with insubordination" distributing "tricolour emblems" in revolutionary gesture in the lecture theatres (57). Grant, the chemist Edward Turner, and A. T. Thomson (whose own son was banned from the school for his conduct during the troubles) were asked to investigate student grievances. All were sympathetic to the students (a fact which angered conservative critics) and saw "ruin impending over the University, if Mr. Pattison remained" (58). Eisdell's allegations make it clear that Grant's Geoffroyan recapitulationist lectures had only served to point up Pattison's ignorance. Eisdell told the *Lancet* that he had

obtained sufficient insight into the higher departments of that science [anatomy], – by attending the lectures of our learned professor of comparative anatomy, Dr. Grant, in whose class I obtained the gold medal, – to convince me that Mr. Pattison, independently of the superficial manner in which he gave his demonstrations, by almost wholly omitting to treat of that department of the science called ‘general anatomy;’ in neglecting to indicate the pathological changes to which the various tissues are subject, and in failing to reveal to us the researches of Tiedemann, Meckel, Serres, Geoffrey [sic] St. Hilaire, and others, into the laws of organization, – did a wrong to the cause of science, which could only be obviated by his removal from the chair of anatomy. This conclusion was forced upon me more particularly by one circumstance amongst others, viz. the “*deplorable ignorance*” Mr. Pattison manifested of the stages through which the brain passes in the progress of its development, when he gave his class to understand that every part was developed simultaneously. Dr. Grant was present when this statement was made, and has confirmed the truth of the allegation (59).

Pattison, playing on conservative feeling, accused Grant, Turner, and Thomson of “caballing with the students” – of siding with “riotous pupils” in a “wicked conspiracy”. Of course, Eisdell’s implicit acceptance of Grant’s superiority, while it bore an eloquent testimony to the profundity of Grant’s transcendentalist lectures, invited a backlash. Pattison held that Grant was a poor judge, and sarcastically recalled in his own defence:

One day when I was in the habit of visiting Dr. Grant, having called on him he directed my attention to a large work which was lying on his table and made the following observations:- “Pattison, if you could only produce a work like that, you would render your name immortal. The GREAT MAN who has published it devoted his whole life to its preparation” (I think he mentioned forty years), “and I should be content to die if I could only leave such a legacy behind me.” Anxious to examine the nature of the book which had excited so warmly Dr. Grant’s admiration, I opened it, and to my amazement I

found that the single subject treated by the author was the anatomy of the beetle!!! In Dr. Grant's opinion the man who spends his whole life on the anatomy of the beetle renders himself immortal, whilst in mine he is convicted of a wilful waste of his existence, which was surely bestowed on him for other and more important purposes. Dr. Grant may therefore honestly believe that because my lectures on anatomy were all made to bear on the great, and to the medical practitioner, the all-important doctrines of practice, that I am an incompetent teacher of anatomy. That I had devoted my time and attention to the idle and unprofitable speculations of some of the German anatomists, and for example, spent nearly the whole session in the attempt to prove an absurdity, viz. that all the bones of the skull are vertebrae, I should then have merited and received the mead of his approbation (60).

That disaffected professors could laugh at Grant's philosophic lectures (even as a last ditch defence) proves their susceptibility in this unstable political climate (transcendentalism in 1830 carried a disconcertingly liberal taint; and Grant's lectures always received their best press in *The Lancet*). With conservatives again worried about subversive philosophy following the July Revolution (and horrified that reckless students could wave tricolor flags in defiant gesture), any exposure of deistic Geoffroyan anatomy at the Godless college could be guaranteed to whip up reactionary feeling. Grant, retiring by nature, refused to reply, but others took up the cudgels in his defence, Turner most vociferously. Edward Turner (1798-1837) and Grant were colleagues and friends; both were strong Continentalists – Grant actually supported Turner's candidature for the chemistry chair on the grounds that his education in Gottingen and proficiency in German would enable him “to keep progress with the rapid march of the science” (61). Turner

for his part hailed Grant as the future “*Cuvier of this country*” (62). He publicly deplored Pattison’s smears and “feeble attempts to throw ridicule on Dr. Grant”, and saw the whole incident rebound in Pattison’s face, since he had proved himself clearly ignorant of the “principle recognized by all modern authorities” (viz. the vertebral theory of the skull) (63). Students too rallied. One was appalled that Pattison had not actually *heard* of Hercule Straus-Durckheim’s *Anatomy of the Melolontha Vulgaris*, the deprecated “beetle” book which was acclaimed as “the best monograph that has been written on any portion of comparative anatomy, and almost the only authority we have on the structure of articulated animals” (64). The students bore ample testimony to Grant’s “amiable character” – imagining it impossible to find a “likelier personage to enact the conspirator” in Pattison’s paranoid delusion of a “cabal”.

Grant was thus unwittingly implicated in Pattison’s sacking and the arrangement and contents of his courses were indirectly affected. From the outset he had included extinct animals in his zoology lectures – events following Pattison’s dismissal now prompted his presentation of a separate “Fossil Zoology” course in the late Spring. It came about as follows. The geology chair remained unfilled at the foundation for want of a suitable applicant, despite the geological interests of founders like Warburton, Birkbeck, and Horner (65). Believing that the Yorkshire geologist John Phillips

was interested in 1831, Horner encouraged him to deliver a geology course in Gower Street during the Spring and apply for the chair. As a mutual friend informed Phillips,

Your election to the Geol. Chair in the L.U. [Horner] deems a matter of certainty – you can have no rival – for ‘after Buckland, Sedgwick, Murchison, Fitton, &c.’ he said ‘who is there?’ – & they are all either precluded or too affluent to seek the office. The reverse of the medal is that the U now guarantee no salary: that you must entirely depend on the popularity of your lectures. (By the by, I believe Grant has a very small class: I understand he is a dull lecturer.) (66)

It was assumed that “Geology being a more fashionable science” than botany or zoology, Phillips would command “a more dignified & larger class, & that in the 1st instance the Fellows of the G.S. will attend *in force*”. Thus it was confidently expected that the Chair would yield £200 p.a. and that Phillips could make up with reviews and popular lectures at the Royal Institution.

Dr Turner is rejoiced at the anticipatn of so useful a Colleague. He is intent on teaching Mineralogy, & Crystallography, geometrically illustrated. He conceives that he may with propriety tackle that branch, yourself Geology properly so called, while Grant will continue to illustrate the latter by lecturing on fossil Zoology (67).

Phillips did deliver his course in May 1831, successfully it seems. But being on the spot brought home to him the university’s frightful condition – the crippled finances and internecine squabbling. The punctilious Horner had now resigned, “fairly scared and worn out”, as Bell said, by the vexatious attacks on him during the Pattison affair. (This at least

affected a saving. His post, prodigiously overpaid at £1200 p.a. – and an affront to the poorly paid professors – was abolished and its function taken over by the Secretary Thomas Coates, salaried at £200 p.a. – which was still more than many professors were earning.) Horner now changed tack entirely and warned off Phillips, advising him that to join while the school’s “affairs are so terribly *embrouillés* ... would be nonsense” (68). With no geologist apparently willing to take the financial gamble of joining an institution whose future seemed in doubt, Grant in 1832 combined with Turner and the botanist Lindley to deliver the course, borrowing fossils from the Geological Society for the purpose (619). Turner lectured on mineralogy, and Lindley dealt with extinct plants (on which he was an authority, having begun publishing his three-volume *Fossil Flora of Great Britain*, co-authored with William Hutton (70)). Grant delivered a “fossil Zoology” course as part of the tripartite agreement. This arrangement existed at least as late as 1836, for in February of that year he informed the Secretary:

I have been lately increasing my materials for illustrating the history of fossil animals and should like, this spring (in the month of April), to give a separate course on this part of my subject as I have done in former years. I think it more consistent with the character of the University than the more lucrative juvenile summer course on Comparative Anatomy and Zoology. I shall not lecture on Fossil Zoology this spring later than April, so that the Geological Course which has been improperly delayed may be announced with reference to this if I am to have part of it (71).

However the following April (1837) he seems to have ter-

minated the arrangement. Why I do not know, but he informed the Secretary: “In order to prevent any misunderstanding in future regarding the lectures on extinct animals, I beg that you will not connect my name in any way with the Course of Geology, either by Fees or by Advertisement” (72). But he did continue yearly with the “Fossil Zoology” course, publishing a synopsis of it in the *British Annual* for 1839 (73) and choosing the subject for his Swiney lectures in the 1850s.

Earnings and Lecture Load

If [the young professor] teaches his science as a chain of demonstrated truths, his auditors are incapable of following him; and he must either bring himself to the level of the humblest illustrations, or surrender the emoluments which are to support himself and his family. He has, indeed, no alternative. He is forced to become a commercial speculator, and under the dead weight of its degrading influence, his original researches are either neglected or abandoned.

David Brewster, in his declinist attack on the iniquities of the professorial laissez faire system at London and Edinburgh (74).

The University’s laissez faire arrangement ensured a direct relationship between earnings and the number of courses and pupils. In this section I will suggest that Grant was stretched to capacity in the college, that his lecture load was increased to maximum in an effort to make ends meet, but that the continuing financial squeeze effectively forced him from the college into the academic market place. He had to

lecture additionally at the London and provincial institutions, in the medical schools, and privately, at his house in Euston Square. The burden of so much teaching took a direct toll, and what with the prohibitive cost of publishing (not to mention the university's tacit discouragement), his original published work dwindled in the 1830s and effectively ceased in the early 1840s. I am not proffering a wholly materialist explanation of Grant's decline, although even this might not prove difficult given the prominent individualism and declinist context of the age. In truth a number of interrelated socio-political factors must be invoked for any rounded explanation. These include his loss of patronage at the Zoological Society after his disagreements with the ruling Council junto; his unwillingness to compromise with the medical monopolists; and his radical allegiance which drew strong ideological opposition and made life for him in the institutions and societies difficult (Chs 5-7).

Before examining the financial exigencies of lecturing, and the evidence that he was forced to hire himself out to institutions, we might compare professional emoluments to get a rough idea of standards of pay across a broad range. By the 1830s the bare minimum for a *typical* middle-class standard of living was an income of £300 p.a. (75). This provided for the necessities (two or three servants, probably not a carriage) and allowed social expectations to be met. Many medical professionals (London doctors, hospital teachers) would

actually have considered this a gross underestimate. But of course Grant was anything but typical, and compared to, say, the socially-aspiring Richard Owen, he assumed a modest existence; he had no wife and family to support, lived in a small bachelor houses and travelled with packed sandwiches (76). Career teachers – rather than gentlemen of means like Lyell, Darwin, or Murchison – certainly scrimped and saved throughout the lean 1830s. While William Buckland was assured of £1000 p.a. at Oxford (77), younger Londoners like Edward Forbes in 1842 struggled on £100 p.a. from the botany chair at King's College, London (713). Nor had the situation materially changed by 1850; Huxley's soul-destroying four-year search for a job is well documented (719). On the reverse side of the coin, medical men, either with an established practice or hospital teaching post, could do well for themselves. Thomas Addison (1793-1860) was Grant's contemporary (MD Edinburgh 1815) and accompanied him to Paris in 1815; and his medical career provides a direct point of comparison. The year Grant received his appointment at LU Addison took the Chair of Materia Medica at Guy's Hospital, and was shortly earning £700-£800 p.a. teaching at Guy's and the Webb Street School (80). Grant's senior Edinburgh colleague, lifelong friend and travelling companion, Marshall Hall (1790-1857), earned £800 during his first year of practice in London (1826), and by 1833 it had topped £2200 p.a., although Hall was admittedly recognized as “the rising sun of the profession” (81). At the top end of the scale, gentleman surgeons at Lincoln's Inn

might earn over £5000 p.a. (Everard Home's average). Benjamin Brodie's private practice alone brought in £6500 p.a. by 1823 (82). Thus one begins to appreciate the financial sacrifice Grant was making for his political principles. Comparison with influential surgeons is of course unreasonable – the Carlisles, Coopers, and Abernethys had wealthy patrons and royal connections (Ch. 5) – but Addison's hospital emoluments or Hall's professional fees show the sort of figures Grant could have commanded had he submitted himself for examination to obtain a licence.

An institutional study tends to confirm that it was difficult to survive solely on the proceeds of lectures on comparative anatomy or natural history in the 1820s and 1830s. At best, one might go for large fashionable audiences at the Royal or London Institutions. In Albemarle Street, for example, Harwood received £50.10s for short courses of popular lectures (about twelve in number) on animals and birds in 1826 (83). He could thus deliver a number of different courses to juveniles and adults during one year. Lectures at the RI were invariably well attended; Lyell could attract 250 listeners, 150 of them ladies (134); Grant, we will see, fared equally well. But serious courses for *students* could only be contemplated by those whose professional remuneration was already high, from a practice or hospital post. The powerful and influential J. H. Green was Professor of Anatomy at the Royal College of Surgeons when he delivered his four-year comparative anatomy course there

(1824-7) (see Ch. 5); while J. F. South (1797-1882) was demonstrator in anatomy at St. Thomas's Hospital when he lectured on comparative anatomy in the 1820s (85).

In other words institutional protection or private subsidy was the rule. Patronage was also important in such cases, and both Green and South were integrated into the Henry Cline-Astley Cooper nepotistic network centred on Lincoln's Inn; thus they were ensured of widespread medical support and a good-sized audience. Owen also provides evidence of patronage at work, both in Lincoln's Inn and among the Coleridgean 'clerisy' generally. His case is directly relevant because he progressed from cataloguing Hunter's collection to teaching comparative anatomy at the College of Surgeons, in the process becoming Grant's chief rival for student auditors in the capital. Owen was supported by a rich corporation and probably received a higher salary than is generally realized. But then social needs made heavy demands on his pocket. Thus the £150 plus £50 "remuneration" (86) he received in 1830 as assistant conservator in the Hunterian Museum was deemed wholly inadequate. Already engaged to William Clift's daughter Caroline, he needed considerably more before he could contemplate marriage. In 1833 he applied to the Chairman for an increase, and his salary was accordingly raised to £300 p.a. in July (87). This was still insufficient, and he only married after being given premises above the museum in 1835. In 1836 he was elected Hunterian Professor and at least by 1838 was receiving a further gratuity of

£100 p.a., giving him parity with Clift (88). In addition, he was the recipient of huge lump sum grants from the sympathetic Anglican managers of the British Association (see Ch. 7) to sponsor his anti-transformist work. Owen was probably never as poor as is often portrayed (89).

Grant could count on no equivalent institutional protection, nor did he enjoy the benefits of the strong patronage system operated from Lincoln's Inn (quite the reverse; the College of Surgeons was one of the reformers' main targets in the 1830s – see Ch. 5). The problem of enticing students into voluntary attendance was familiar to all observers of the University. If anything, the rival Tory-Anglican King's College fared even worse. Lyell was thwarted in his effort to make geology pay by the bishops' insistence on banning ladies (who made up the bulk of his auditors). He returned to authorship, telling Fleming in 1833:

I regret that the bishops cut short my career at King's College, as I should have had a splendid class this year, and thrice as profitable as at the Royal Institution, but there seems no way of having a large audience but by one of two methods – an academical [compulsory] class, or one open to women as well as men. Grant lectured *gratis* at the Zoological Society to overflowing rooms, but the moment he began at the London University, for a trifling fee, only about eight or ten students came. Yet people will still buy dearish books, and though willing to hear Turner lecture gratis on Chemistry or Geology, they will not pay at the rate of 1s. a lecture to hear him (90).

And the abolition of entomologist James Rennie's Chair of Natural History at King's the following year for want of

students was a caution to all.

Failure to make comparative anatomy compulsory for MD students at Gower Street would not have been so bad had the school's intake matched early optimistic projections. Two thousand pupils a year was happily forecast, putting London on a par with Edinburgh. Had this been realized, Grant's story might have been different. After all, Jameson's class was not required for medical students (at least until 1833), yet with 2000 students in the university (900 in medicine alone) he could always count on a sizeable audience: for instance he attracted 200 auditors in 1826 (91). But at London quotas fell far short. In 1829-30 total intake only reached 630, and in the years immediately following it actually fell (92). Attendance figures and fees up to 1851, when Grant was awarded a stipend, are given in Table II. From 1831 until 1851, that is, while he remained reliant solely on fees, the total number of students for all his classes taken together averaged 33 per year, and his share of the fees amounted to c. £106 10s per annum. While he fared better than the professors of English, Philosophy, or German (93), he was very much the poor relation of colleagues like Lindley (who in a good year, say 1834-5, could make £379 before deduction), Turner (1834-5, £813), and Sharpey, who took £686 in his first year (1836) in the Physiology chair (94).

The inequities of professorial laissez faire were only too apparent to the declinists. Brewster, who had recommended

Grant for the chair, was undoubtedly aware of his wretched situation. In his *Quarterly* declinist broadside, Brewster bemoaned the fact that so few chairs existed to induce “the young philosopher” to sacrifice “all other professional expectations”, and any benefit from those that did was “far outweighed by the baneful influence which such situations produce” (95). Either a savant teaches serious science and surrenders his emoluments, or he becomes a commercial speculator and abandons his researches. One solution Brewster proposed for London University was to split the rich chairs, employing both a popular lecturer who might draw the crowds and a great philosopher who would bestow dignity. This was

YEAR	Total No. Comp.Anatomy +Zoology students (Fossil Zoology in brackets)	FEES (£)	GRANT'S SHARE (£)
1833-4	25	93	93
1834-5	34	121	110.10
1835-6	34(+11)	127	113.10
1836-7	53	227	163.10
1837-8	27(+5)	104	102
1838-9	27(+6)	145	122.10
1839-40	35	116	108
1840-1	35(+14)	153	126.10
1841-2	17(+11)	118	109
1842-3	33(+11)	171	135.10
1843-4	22	94	94
1844-5	23(+15)	171	135.10
1845-6	(no figures available)		
1846-7	27(+8)	116	108
1847-8	33	102	101
1848-9	25	82	82
1849-50	23	87	87
1850-1	7(+4)	39	39

Table II

fine, except that few chairs reached the £800 or £1000 p.a. threshold conceived by Brewster. And applied to the poorer chairs it would prove catastrophic; Horner's similar idea of introducing a popular lecturer on the cow, sheep, and horse, had Grant up in arms, and rightly because it would have taken his livelihood.

Even before his “guarantee money” was rescinded in 1831 Grant was suffering financial “anxiety” (his commitments were heavy in the early years: he was paying for preparatory material, class medals, etc.). After requesting the remaining £100 of his “guarantee” in March 1831, he told Horner that “although I have increased my labours in the University nearly to the utmost of my strength, the proceeds of my Classes are yet far from affording me the means of subsistence” (196). He delivered a staggering 197 lectures in nine months at the University in the 1829-30 session, and thereafter kept up an average of 200 per year, a number he was still delivering in the 1850s (97). He had not, he told the Secretary in 1842, “either from bad health or other causes, omitted a lecture or been ten minutes too late at my post, for now nearly fifteen years” (98) – the same boast was being made in 1850, although sheer hard work could not save him from “absolute penury” (99) when attendance figures began plummeting after 1848 (see Table II). His solution in the 1830s was to take private students, deliver extra-mural lectures and courses at the London medical schools, and vie

for lucrative posts at the fashionable institutions. Only by lecturing outside college, he claimed, could he afford to remain inside; this proved an eminently workable arrangement until the Council, worried that the extra-mural classes of professors would prove detrimental to the university, tried to curb this subsidizing activity.

Grant's Role Outside the University:

The London Schools, Societies, and Institutions

The issue was brought to a head on 22 September 1837 when the Committee of Management of University College passed a resolution refusing Grant permission to deliver a course of lectures at Marshall Hall's Sydenham College. Infuriated, Grant the same day drafted a long reply to the Secretary, spelling out his position.

In reply to that communication informing me that my engagement to lecture at the school "would be inconsistent with my duties as professor in University, College", I beg leave to observe that from the period of my appointment to the University in July 1827 to the present moment I have never heard that even a wish had been entertained by our Council or its Committees to interfere with the avocations of the teachers without the walls of our unendowed establishment.

When I left Scotland and my professorial prospects, to conduct the humble department entrusted to me in the University, I anticipated (what I cheerfully encountered after the third session of our career) that I might be compelled to undertake duties out of the University to enable me to continue them in it, and several Members of Council (especially Mr Horner and Mr Tulk) gave me every assurance that our governing bodies would never countenance any interference with the private avocations of Professors or their labours *extra muros Academicae*.

Relying on these liberal assurances which have

never till now been contradicted ... I have already accepted of many engagements to lecture in Scientific Institutions and Medical Schools without the walls of our College.

The engagements which I have contracted for this session at the Sydenham and the Aldersgate Medical Schools of Medicine, and the vicinity of the Sydenham to our College (which you mentioned to me as a ground of objection) is almost exactly the same as that of my private residence, where I have already delivered two Courses of lectures on Human Physiology, without even a fear of interruption.

As my Course in University College consists generally of more than 130 lectures, embracing the whole extent of my subject, and those which I deliver without our walls never exceed 12 or 15 lectures, confined to some limited branch, the latter can never operate to the disadvantage of the University Chair...

... I feel confident that the Committee will ... reconsider a resolution so seriously affecting the rights enjoyed by our teachers since the first foundation of University College, and so ruinous to the prospects of our unendowed and unprotected Chairs (100).

When Wakley in 1836, seeing Grant advertise lectures on comparative anatomy at the Hunterian School in Great Windmill Street and the medical school in Aldersgate Street, inferred that the demand for instruction in this “deeply interesting branch of science” was on the increase (101), he had misunderstood the situation. Demand was not increasing; Grant was having to spread his courses to attract enough paying students.

But financial considerations themselves did not dictate which institutions he actually favoured. Also, on a more mundane level, his dealing with the provincial Literary and Philosophical Societies show him to have been unwordly where money was concerned, and unaware of his own worth (or at

least, too conscientious). In the 1830s he delivered summer courses of varying length (six to twelve lectures), almost always on invertebrates, at the provincial institutions of Leeds, Sheffield, Manchester, Liverpool, Hull, and Birmingham. Yet these lectures were not always great money-spinners; it was not that they were underpriced, but rather that his fees remained invariant, whatever his distance of travel.

Concluding arrangements with William Hutton at Newcastle in 1838 he wrote:

I could commence a course of twelve lectures on the Structure and History of Invertebrated animals on Monday the 15th of July next, and continue with three lectures, or two, per week, as may suit you, and at the usual fee of five guineas a lecture. I begin at the lowest grade of the Animal Kingdom and proceed upwards, and as I carry no specimens (but only diagrams) with me, the lectures could have little interest to your Society unless you have access to some small collection of corals, shells, and insects. I have no other first course to propose. The above sum includes all expenses, and the details of the prospectus will be forwarded should the arrangement be concluded (102).

From the five guineas Grant had to print his prospectus and pay his fares. Dr James Russell and the Managers of the Birmingham Philosophical Institution felt that Grant's fee was wholly "inadequate as even a business remuneration, especially as a double railway fare had to be subtracted from each five guineas; for at that time a return ticket was not available beyond midnight [when Grant set off back to Euston]. Accordingly they added, from their own pockets, the total railway fees (103), which Grant however "absolutely refused", even under duress; nor would he allow the managers

to wine and dine him afterwards, preferring a bag of sandwiches as he set up his diagrams. So the danger is of imputing materialistic motives, when he was neither Brewster's commercial speculators nor a circus showman, but a conscientious 'careerist' lacking professional advantages.

An additional factor emphasizes the naivete of assuming that Grant exploited all institutions alike for commercial gain. It is that he showed marked preferences; and his allegiances are an indicator, not only of his appreciation of respective proprietorial ideologies, but of the direction he conceived his career taking. We must also remember that at the private institutions and learned societies a two-way process operated – career enhancement depended both on choice *and* acceptance. Grant's overriding interest in, say, cephalopods and fossil zoology, might have made the Zoological and Geological Societies professionally relevant, but incompatible managerial ideologies could nevertheless have made him uncomfortable in Bruton Street or Somerset House. Morrell argues that the proliferating institutions at this time were functionally effective in different ways for individual scientists (104). We have to determine which societies were deemed desirable or hostile by which group and for what reason. Obviously a prosopographical approach is helpful here. We can reveal scientific orientation and social expectations by analysing the collective biographies of managers, proprietors, or fellows, depending on electoral procedures or constitutional make-up (i.e. on who dictated

policy). Consider the Royal and London Institutions, which appear superficially very similar. The Royal Institution (f. 1799) in Albemarle Street might have begun as a social vehicle for ‘improving landlords’, but electoral changes in 1811 shifted power from the agricultural aristocracy to the professional classes. By the mid 1820s control rested with the Utilitarian Whigs, those holding what Morris Berman calls a legal ideology of science, i.e. those who appreciated science as a professional tool in organizing and administering a smoothly-functioning rational society, in which social ills were to be eased, the medical profession upgraded, and education promoted. The RI was thus managed by the Benthamites responsible for meeting middle class needs with the founding of LU and mitigating working class dissent through the literary palliatives of the Society for the Diffusion of Useful Knowledge. Berman reasons that the RI helped to articulate

ideologies of science which would ultimately usurp the aristocratic one. Certainly the RI was fashionable and elegant, but fashion and elegance were not its goals. By bending science to entrepreneurial and professional purposes, the RI was the opening wedge in a major ideological shift (105).

The ‘improving landlord’ gave way to the ‘improving physician’, for whom science represented the instrument of social progress in the Reform years. Berman’s scenario is attractive, helping to explain why Grant should prefer the RI to, say, its rival, the London Institution. The commercial/ colonial/industrial elements never achieved dominance at the

RI, and indeed after the 1811 reforms the business interest abandoned it to the professional classes. The merchants, bankers, and City proprietors seceded in 1805 to form their own London Institution, eventually (1819) to settle in a fine building in Finsbury Circus. The primary aim of the LI was to expound the commercial application of science – to “bring together the natural philosopher and the financier” (106). Here science and commerce as handmaidens were to lead, and civilization, virtue, and religion would follow (to the four corners: the East India Company had an interest in the LI’s affairs). While it catered to, e.g., phrenological tastes and ran elocution lessons (107), it was far removed from a Mechanics’ Institute, and offered little ‘hands on’ experience, taking a more entrepreneurial approach. At the same time it tended to amateurism; indicated by the fact that stock lecturers like E. W. Brayley would free wheel from one subject to another. Given the City interest of the founding fathers, it is understandable that zoologists ignored the agricultural applications of their subject, but their dilettantism was an obvious concession to popular taste. Harwood’s lectures in 1826 resembled a zoo guide, Rennie’s in 1831-2 extolled the virtue of butterfly hunting, while in 1833-4 Brayley introduced audiences to quinarian methods of cataloguing their bird or beetle collection (108). The philosophic anatomist was ill-placed here; Grant delivered one course (on invertebrate structure and classification in 1835 (109)) and no more. Systematics and taxonomy, Grant’s

stock-in-trade, were unsuited to Finsbury's popularist needs. Being no Brayley or Harwood, he was incapable of vulgarizing (as the extant handbill shows). It is unlikely that proprietorial ideology *per se* offended him (after all, his brothers were East India Company men) – more probably he found the Institution's rank amateurism simply uncongenial.

The RI better suited Grant's Utilitarian tastes. At Faraday's request, he delivered ten Friday Evening Lectures between 1833-41 (110). As *gratis* lectures preceding the *soirée*, these were supposed to be of an “easy and agreeable nature” (111). Still Grant's were comprehensive, covering philosophical themes dear to his heart: recapitulation, the progressive complication of organs, Geoffroyan vertebral osteology, and so on. As with all *soirée* lectures they were written up in the *Medical Gazette* and numerously attended. Data from the RI Archives show that Grant's audience ranged in size from 212 to 428 persons (112). Even lectures on Geoffroyan esoterica, like that on 6 June 1834 “On the Development of the Vertebral Column”, attracted 378 – probably more a function of the *soirée* to come than the osteological *hors d'oeuvres*. He also ran his course on the invertebrates in 1834 (113). While in 1837 he was elected to the triennial Fullerian Professorship of Physiology (but only after Owen declined the post, see Ch. 5), beating rivals like R. B. Todd (King's), Herbert Mayo (Middlesex), Samuel Solly (St. Thomas's), and Owen's protégée Thomas Rymer Jones (114). As Professor under Fuller's deed he was paid £50 half-yearly

and delivered courses on the nutritive, motor, and sensory functions of animals (in 1837, 1839, and 1840 respectively (115)). For a time Grant evidently identified with the RI's professional and Utilitarian managerial policy. But he had little more to do with Albemarle Street after 1841; and in the 1850s the Secretary Rev. John Barlow was horrified at the prospect of Grant of all people filling the post a second time. Barlow also refused his offer of (transmutationary) lectures on "Palaeozoology" (116); clearly Grant's extreme naturalism and transformism were more appealing to radicals than the Broughamite managers of the RI. To observe the ideological tensions that were generated, however, it is necessary to switch our focus onto the learned societies.

The Learned Societies: Royal, Linnean, and Geological

While a dominant coterie might define the ethos of an institution, to miss the small but significant radical element would be to do an injustice to the fine texture of social history. Not even the Old Lady herself, the Royal Society, was immune to infiltration. Enough radicals were Fellows in 1836 to support Grant's candidature. Where, by comparison, Owen – as a Peelite anti-transformist – had polled support from the powerful Coleridgean lobby in 1834, the gentlemen surgeons of Lincoln's Inn, many with appointments to the Crown (117), Grant's backers were preeminently progressive physicians and radical reformers. They

included the Utilitarian William Tooke (1777-1863), founder of LU and reformist MP for Truro; the Unitarian mining entrepreneur John Taylor (1779-1863) whose family had working class sympathies (118); the ‘improving physician’ John Bostock (1773-1846), critic of the College of Surgeons; and the brilliant but eccentric John Elliotson (1791-1868), phrenologist and mesmerist forced to resign from LU (119).

But Grant published nothing in the *Philosophical Transactions*, and one can appreciate his difficulty when Coleridgeans like J. H. Green were helping Owen to place his anti-Geoffroyan papers with the Society. Grant attended meetings but rarely refereed papers (120); unlike his clubbable colleague William Sharpey, he took no part in the administrative affairs of the Society. Wakley, always one to spot the action of privilege and conspiracy, insisted in 1850 that Grant and Hall had been “systematically excluded from every place of distinction in the Society,”, and the old political campaigner demanded that the deplorable state of the Society’s affairs would only be remedied when such scientists of *honour* were elected to the Council. But Grant’s abhorrence at the caballing left him constitutionally incapable of co-operating. When Thomas Bell invited him in 1849 to join the Committee of Zoology and Animal Physiology, he flatly refused to aid its “secret and invidious functions” (121) and inveighed against the Society’s despicable treatment of his friend Hall (not that he lacked grievances of his own – I will cover the award of the Copley Medal to George

Newport for work plagiarized from Grant in the next chapter).

His attitude towards the three younger societies – the Linnean (f. 1788), Geological (f. 1807), and Zoological (f. 1826) – was more positive. Again, however, events were to prove disappointing, as the ethos of each turned out to be deeply inhospitable. He certainly anticipated a long-term commitment, compounding for life membership of all three societies, at no inconsiderable cost (his composition fee for the Geological in 1831, £31.10s, was more than his previous term's takings from comparative anatomy classes! (122)). He became a Fellow of the Linnean Society on returning to Edinburgh in 1820 and was elected to the Council after coming to London in 1829. The Society however was devoted to descriptive natural history and addicted to the peculiarly English quinarianism. Taxonomic nit-picking and circular reasoning by government officials had no obvious appeal for a philosophic anatomist. And the larger views of typical quinarians like the author and naturalist William Swainson (1789-1855) would have positively alienated him. Swainson denied the classificatory value of internal structure (diminishing the role of comparative anatomy) and was suspicious of the French school – jealous of its patronage and dismissive of its product, whether it was Lamarck's linear series, Cuvier's law of correlation, or its “cold, ill-concealed spirit of materialism” (123). Although Grant was elected to the Council in 1829, he was inactive (124) and removed the following year. He published nothing in the

Transactions and thereafter took a back seat (125).

The Geological Society had a distinct social composition. By the 1820s the place of the original mineralogically-orientated founders had been usurped by an enterprising group of wealthy gentlemen careerists (126). It had become, in Morrell's words, "a gentleman's geological club dominated by a coterie". Though this oligarchy was based on merit and not rank, gentlemen of secure income, such as Lyell, Murchison, Broderip, Fitton, Greenough, and Darwin, aided by academics such as Sedgwick, Buckland, and Whewell, dominated the meetings of the Society, dictated its social tone, and engineered key appointments such as the Presidency" (127). During the critical Reform years 1830-40 many gentlemen of the Geological (Lyell, Sedgwick, Greenough, Whewell, Buckland, Owen) were *actively* hostile to Lamarckism, which as a socially-subversive, theologically-disturbing philosophy obviously ran counter to their programme of political stabilization; and a well-connected transformist like Darwin wisely kept quiet during meetings, whatever his private feelings (128). Grant's heretical views, unlike Darwin's, *were* known, and we can document the procedures adopted as members closed ranks on him. From the first he took a lively interest in the Society. He was elected a Fellow in 1830, and to the Council while Turner was Secretary in 1832 (129) (ironically the date of the appearance of Lyell's anti-Lamarckian volume of *Principles*). But although 50 entries for

Grant in the General Minutes over the period 1830-45 testify to his staying power (he brought a succession of students and guests), I doubt that he was comfortable or his position ever more than peripheral. Particularly since officials like Owen and Buckland in the later thirties out-manoeuvred him on sensitive issues bearing on transmutation, blacked his papers, and engaged him in acrimonious exchanges (Ch.7).

One lesson to learn is that these societies cannot be seen as social monoliths. Grant did retain his foothold, at loggerheads with the dominant coterie maybe, but probably supported by rank-and-file members like Turner and the Treasurer John Taylor. Although the historiographic emphasis of late has been on the social role assumed by the elite spokesmen of the Geological or British Association, viewing the institution from a minority radical perspective makes one aware that even supposedly socially-homogenous societies suffered inner tensions, which in less dramatic ways mirrored the events in the troubled political arena. The managers might have presented the GS as socially cohesive (obviously they were canvassing for respectable support and patronage from the Exchequer); and they did, as Morrell and Thackray say, contrive to keep out working-class elements and reserve key positions (130). But for all the public relations the struggle within the Society between Peelites like Buckland, Whewell, and Owen, and the radicals should not be overlooked, particularly as the competing ideologies became constitutively embodied in rival scientific formulations (Ch.6-7).

**Zoologists, Zoological Organization,
and the Zoological Society**

Judged by the time and effort he put into the Society, Grant saw his career interests best served by the new Zoological Society (elected FZS 1828). He assumed numerous managerial responsibilities, delivered extensive *gratis* lectures to the Fellows, and chose its journals as his publication medium. That he was willing to devote considerable time and effort to the Society's affairs for no financial return when he was already struggling at the University testifies magnificently to his commitment. His *forty* lectures over *four* months in 1833 "On the Classification and Structure of Animals" (131), delivered to a huge audience in the Society's Bruton Street Museum, were unremunerated. The same was true of his "Fossil Zoology" course the following year (132). (These were the first major lecture series delivered to the Fellows.) What makes this more piquant was that his audience was largely salaried government officials, wealthy avocationists, and hospital staff. No wonder Lyell – who duly noted the irony – despaired of earning enough from lecturing and returned to the gentlemanly pursuit of writing. Committee time too was invaluable to an academic penalized by laissez faire. In 1830 he served on the newly-formed Committee of Science and Correspondence (133), whose purpose was to discuss animal experiments, exchange intelligence with foreign members,

promote the importation of rare and useful animals, and draw up reports. Such was the need for regular *scientific* (rather than exhibitional) meetings, and the continual criticism of the Council on this score, that the bimonthly meeting of this Committee eventually became the forum for the reading and publication of papers. He subsequently served on the Publication (1833) and Museum (1834) Committees appointed to look into the subject of providing a new and larger museum (134) – again a needling point with declinists and reformers. The Society was undoubtedly useful to Grant. It possessed a menagerie and gardens in Regent's Park, where he could take his classes. It was also crucial from a resource aspect, since the Piccadilly museum was a storehouse of exotic cadavers for dissection, many of them received via the Society's – or its wealthy patrons' – colonial, military, or East India Company contacts. By 1828 the Museum already housed 600 mammals, 4000 birds, 1000 reptiles and fish, 1000 testacea and crustacea and 30,000 insects (135). Grant dissected tropical cephalopods here, and anatomized the grampus, condor, and Indian tortoise for the *Proceedings*. In 1833 he submitted eleven papers on Society material for publication. So there is unequivocal evidence that he had established himself at Bruton Street between 1830-4: taking on an administrative load and teaching duties, and favouring the Society's journals. By 1833 he looked destined to play a major role, yet in May 1835 he suddenly retreated, infuriated at managerial irresponsibility, after a calamitous rift among

the Fellows following what appeared to some as an attempted purge – all of which, conveniently, left the more ideologically-aligned Richard Owen as the main custodian of the Society’s scientific wealth.

Exactly what lay behind this move is even now difficult to tell. Owen’s complicity, and the specific problem of power sharing with a Lamarckian, will be treated in Ch. 5. Here I want to try to get a feel for the managerial/methodological points of friction between conservative zoologists and reformers, by means of a general study of the organization of zoology in the late 1820s and early 1830s, from which we can understand the dominant ideology of the ZS’s founders. In short, reformers generally urged greater patronage and protection, and on methodological grounds deplored the particularist, nit-picking taxonomic obsessions of zoologists. The presuppositions of conservative zoologists, on the contrary, inclined them towards detailed, empirical, descriptive work, and this anti-theoretical bent suggests that the environment in London was potentially hostile to philosophic anatomists and transformists.

The mid 1820s during the break-up of the Banksian Learned Empire was a time of rapid institutional differentiation and reorganization. It saw the establishment of the Zoological Club of the Linnean Society (1823-9) and Zoological Society (1826) – both testifying to the inadequacy of the Linnean to meet either the scientists’ or improving aristocrats’

zoological demands. Journal titles proliferated; witness the *Zoological Journal* (1824-34) and ZS's *Proceedings* (1831 on) and *Transactions* (1835 on), and also J. E. Gray's *Zoological Miscellany* (1831-45) and Owen's own short-lived *Zoological Magazine* (surviving only six numbers in 1833) (136). It wasn't simply that zoology was swept along on the tide of fashion. True, Tory magistrate W. J. Broderip, while treating genteel *Quarterly* readers to a travelogue of feasts at the Zoological Gardens in 1836, judiciously warned the malcontents not to rock the boat, since "it is the gale of fashion, more fickle than any 'i' the shipman's card", that has hitherto borne the Society so prosperously along" (137). But avocational demand for lectures on exotic beasts also reflected Britain's expanding colonial empire. Rev. William Kirby, on inaugurating the Zoological Club, talked of "the zoological treasures that our ships, whose sails over-shadow every navigable sea, are daily bringing to our ports" (138). With the founding of the ZS, circulars were immediately sent via the Colonial Office to solicit the help of the colonial Governors in collecting and returning specimens. Proprietorial interest in the foundling ZS also differed from that of the Linnean, partly centring on the potential for breeding the exotic beasts to supplement the gentleman's table.

None of the zoologists we discuss were professionals *stricto sensu*; i.e., salaried from private or public funds to carry out full-time research, train students to professional

standards, or in general create a marketable commodity called ‘Zoology’ (139). Even Grant and Owen, as full-time academics, met only certain of these criteria. (Grant was primarily employed to embellish the liberal education received by proprietors’ sons; the surprise is that he turned out such a string of excellent protégés. Owen was hired to enhance the College of Surgeons’ reputation by cataloguing Hunter’s collection.) And even while many zoologists adequately fit into Roy Porter’s pre-professional ‘careerist’ category, *socially* they were markedly distinct from his leisured geologists. Indeed, the pedantic, bureaucratic front presented by careerist zoologists probably warned off the gentlemen geologists, which as a group never had much to do with the ZS (140). A limited prosopographical study of the core group of zoologists – the officers of the Zoological Club, most frequent contributors to the *Zoological Journal*, and scientifically-active members of the ZS (141) – reveals a striking pattern. Significantly, none were educated in Edinburgh, where the wealthy sent their sons to receive a liberal education. Most came from affluent families, were educated at the London hospitals or Oxbridge, and were employed full-time in law, medicine or government service. The majority fell into the last category: J. G. Children and J. E. Gray were librarians at the British Museum, W. S. Macleay was attaché to the embassy in Paris, J. F. Stephens worked at the Admiralty, and Joseph Sabine became Inspector-General of Taxes (142). This respectable group of avocationist-cum-careerists envisaged its role in providing exact

classification and detailed description (143); although this activity took the brunt of reformist criticism it does accurately reflect the mainstream position. It was also politically important. The mania for naming the hordes of colonial insects reflected more than a *metaphoric* capitalist expropriation; in Kirby's Presidential view, naming was nine-tenths of the law of possession:

Names are the foundation of knowledge; and unless they have "a name" as well as "a local habitation" with us, the zoological treasures that we so highly prize might almost as well have been left to perish in their native deserts or forests, as have grown mouldy in our drawers or repositories. But when once an animal subject is named and described, it becomes ... a possession for ever, and the value of every individual specimen of it, even in a mercantile view, is enhanced (144).

The political split over the future financing of zoology was already marked in the 1820s. The Dissenting lawyer and Criminal and Poor Law reformer J. E. Bicheno urged a protectionist policy. As he reasoned, "A people left to discover and pursue its own interests, without any further interference of the sovereign authority than as it protects property, is not likely to take the lead in any science which does not administer to its immediate necessities" (145). He admitted that England stood pre-eminent in technology because its development depended on entrepreneurial initiative and free-market operations. France was a timely reminder that central bureaucracy could smother ingenuity and enterprise. Zoology on the other hand *flourished* in France where the engines failed precisely because state patronage compensated

for lack of market interest (a point the declinists were also to belabour). Protection was imperative outside the entrepreneurial sphere or, worse, where zoology's 'marketable price' was likely to end up being judged by its value in pest-control. Bicheno's strong reformism was by no means typical (146)*, and was in utter contrast to the fierce individualism of his successor in the Zoological Club Chair, the Old Etonian J. G. Children, who pointed

to the growth and progress of that young but promising child of British energy and science, the Zoological Society. It is a glorious feature in the philosophical character of Great Britain, that whilst in foreign countries Science owes most of her success to the fostering care of Royal patronage, or the protection of executive power, – *here*, with faint exceptions, "few and far between," she relies on her own resources; and, unlike the creeping parasite, raises her head in independent dignity by the individual exertions of her disinterested cultivators, who, loving her for herself, seek only to accelerate her progress, and establish her empire in the human mind on the firm basis of immutable truth. To such an origin the Zoological Society may proudly assert her claim; not one shil-

*Actually the issue did not split strictly along party lines, but was confused by the overlapping question of declinism. Thus William Swainson thought that every "reflecting mind" must look with "fear and dread" on "that 'radical reform' now so loudly called for" (147), and on "hasty" revolutionary changes which defied both natural and moral laws of gradual progress. But even as a politically-centrist declinist he too demanded greater protection and patronage, calling attention to the extraordinary progress under the French pension system, and like Bicheno he deplored the lack of *philosophic* research (by which he did not mean Parisian style so much as quinarian). Yet this was no radical attack: he passed over LU's "poorly supported" efforts to teach academic zoology, decried political reform, and urged the ancient established universities to take the lead and endow respectable chairs of zoology.

ling has been drawn from the public purse for its support: and could it condescend to ask such aid, I for one would raise my voice against the humiliating petition ... (148).

On methodology the split was equally discernable. Bicheno's reformist exception proved the rule – that zoology in London was empirical and particularist. Bicheno admitted the importance of “systematic and technical Natural History”, but excessive attention to nomenclature and unprofitable detail, to the detriment of “the higher ends of science”, was myopic and gave zoologists “a puerile cast” in the public eye (149). He proposed a remedial “subdivision of labour ... as in ordinary occupations”, which would entail splitting zoologists into three ‘classes’: the collectors/collators, anatomical experimenters, and generalizers.

So social reformers like Bicheno stood on a distinct managerial-methodological footing. They conceived zoology growing in liberal directions supported from State or Crown coffers, and welcomed help from comparative anatomy. Empiricism on the other hand was tied to individualism as the ideology of Kirby's *status quo* faction, presumably as an immunization measure against the disruptive influence of Continental ‘philosophical zoology’. Even centre-right declinists like Swainson were wary of ‘Continental’ comparative anatomy; while the more reactionary Kirby was deeply suspicious of ‘medical gentlemen’ who cultivated the sciences, presumably remembering the uproar over William Lawrence's transgressions (150), and later in perhaps the

most backward-looking of the Bridgewaters Kirby decried the “utter irrationality” of Lamarck’s godless materialism (151).

Of course not all abstraction was shunned. The quinarian system was passionately embraced, as any survey of the journals shows (152). But when Club members – Kirby, Brookes, Thomas Horsfield, N. A. Vigors (153) – did praise Macleay’s circular system or his analogy/affinity distinction, it was always as an aid to classification. Nonetheless the system’s popularity made Fleming feel quite out of place on a visit to London: “My refusal to embrace the *Quinarian* nonsense of Macleay made me the object of the virulent persecution of the cockneys. These things joined with the anti-Edinburgh spirit of London ... have placed me rather as an outlaw than as one who has made great sacrifices for science” (154). By extension, a fellow Edinburgh-educated Lamarckian dichotomist like Grant would have been seen in an antagonistic light by Macleay’s sympathisers. Actually it was worse for him, because of his materialist bent. Any metaphysical sentiment that was expressed by Club members was antithetical to the materialism of the Plinian group. Thus the *Zoological Journal*, despite its declared empirical aim, opened with one of a series of articles by J. O. French on brute instinct, in which he distinguished the moral actions of men from conscious behaviour in animals (155). Lamarck might have been “venerable” (156), but only because of his conchological classification, which this group was eminently

suited to appreciate. His transformism was abominated, while Geoffroy's "Peculiar views" on crocodile descent were dismissed by the *Zoological Journal* as a mere curiosity (157).

So what Herbert and Rudwick claim for Darwin was equally applicable to Grant – he could have found no audience for transmutation in the London societies (158). But underlying this, as we have seen, he would have been opposed to the unreformed zoological community on fundamental methodological and theoretical grounds; and on top of this his extreme naturalism and secularism may well have pushed him to the left of fellow reformers like Bicheno who likewise disagreed with the Tory majority on larger policy matters. So although it was as a result of administrative disagreements that he lost his foothold in the ZS, the ideological divide cut much deeper, and managerial policy was only a special case of a larger and more fundamental political split in the Society. Undoubtedly, too, the differences were exacerbated by the aristocratic social base which distinguished the ZS from, say, the GS. Without disputing John Bastin's claim that Club members were influential in founding the ZS (Vigors' "unceasing exertions" on behalf of the Society were lauded at the time, and he quickly took to lecturing Fellows "on the principles of the Quinary System" (159)), nonetheless this does not cut to the ideological heart of the *reason* for the Society's foundation. Enormous interest was shown by the landed gentry and improving aristocracy in the project – and not unexpectedly since their devotion to field sports gave

them an overriding interest in game management (160). Hence Sir Humphry Davy and Sir Stamford Raffles originally envisaged a Society “bearing the same relation to Zoology ... that the Horticultural does to Botany” (161), i.e. it was designed to have “practical and immediate utility to the country gentleman”. Therefore gentlemen of weight and “*honorables*” were the first actively canvassed for their support. The main object of the Society in 1825 was conceived as the domestication of exotic beasts to stock farm yards and ornamental gardens. So it didn’t set out to be *simply* a society for the disinterested study of animal life; there was an active scientific interest, but it was primarily canvassed as a kind of clearing house for aristocratic stock. And to secure patronage members were promised the pick from among the imported beasts for their private woods. The benefit to wealthy patrons was extolled by Davy, a prime mover among the gentry on the Society’s behalf. He broached the idea of a Gardens with Sir Robert Peel, possibly while shooting on Peel’s estate, promising him that “the subscribers should be furnished with the means of making private experiments at a reasonable rate either by being furnished with young animals or the eggs of such birds and fishes as they wished to introduce into their own wood, pond, or farm yard” (162). Even in 1832, as the zoo’s armadillos bred again, hopes ran high of them being naturalized and appealing to a gentleman’s palate. Zoologists after the bourgeois invasion might have laughed at such pretension (163), but as the first prospectus

pointed out, given the startling successes in the past (peacocks, swans, turkeys, carp), the idea was far from ludicrous. Anyway, this leaves no doubt about aristocratic proprietorial interest or the ideology of its landed backers, and shows *why* the Society was distinguished from the GS. The nobility was from the first active in the society.

The Marquis of Lansdowne was elected the first President, and the nobility sportingly stocked up the new Regent's Park Gardens: the Duke of Bedford presented a llama, Lord Auckland a leopard, the Marquis of Hertford some home-bred kangaroos and a Russian bear, and the Earl of Mountcharles a pair of emus bred at Windsor (164).

The above also suggests why the Society concerned itself more with breeding and exhibition than scientific study (and why reformers and declinists should line up to condemn the Council on this score). Anyway, *because* of its exhibitionist bent, the Zoological immediately became one of the wealthiest London societies. By 1828 the Gardens were attracting 130,000 visitors a year (165). The 1829 audit showed a healthy balance of over £2000 (after hefty deductions for building expenses), and investment by the Council began (166). In 1835, when attacks by radicals began to peak, it had a "vast" income, with gate-takings and subscriptions totalling £18,000 p.a. (167).

Radical criticism varied, of course, according to the political standpoint of the critic. Wakley's was levelled from a working class perspective; he objected to the proprietors' monopoly on Sunday tickets, resulting in the

exclusion of working men on their only free day (168). More substantial opposition inside the society was levelled at the management's mishandling of affairs, their monopolistic control of power, and failure to carry out the scientific aims of the society. Attempts by officers to remove critics like Grant and the MP Robert Gordon in 1835 from the Council became a rallying point for the opposition and Grant's case became a *cause celebre*. Wakley vigorously resisted attempts by the "malignant and odious junto" to have Grant balloted out (169). Wakley accused the much-disliked autocratic jobber Joseph Sabine, one of the Vice-Presidents, of engineering the removal of opponents, and the Council clique led by Sabine of administrative abuse (office bearers were practically self-perpetuating and immune to electoral removal). As Inspector General of Taxes Sabine was, it is true, a natural Wakley enemy; also *a priori* he was suspect, having already been forced to resign from the Horticultural Society in 1830 rather than face censure for mismanagement. Reformers had a good case, for funds destined for the museum had been funnelled off for the Gardens. But for all that Sabine did more accurately reflect the ideology of the landed backers, who were more interested in breeding exhibits than funding research. Grant and Gordon deplored Sabine's extravagances and effort to turn the Gardens into a "raree show", arguing for greater development of the Society's scientific potential (that is, of the museum). The strength of feeling on this point was brought home by the declinist broadside; Swainson

in his 1834 *Discourse on Natural History* weighed in with his own attack on the rampant mismanagement, censuring the illiberal managers of the ZS, ruled by despotic “presiding judges” who engineered appointments and blackballed papers. He too accused them of diffusing rather than advancing zoology, and of niggardly behaviour on scientific matters. He considered the “poor collection in Bruton Street” (i.e. the museum that Grant was working to augment) a disgrace to such a wealthy Society (170).

The reserved Grant again refrained from publicly bickering; rabble-rouser Wakley suffered no such qualms and rushed to his aid:

As for Dr. GRANT, his character, his discoveries, and his great fame, render him the main pillar in the institution. Of course that gentleman will consult his own dignity by not attempting by any personal exertion of his own to thwart the machinations, or expose the intrigues, of the despicable faction who have acted so ruinously for the interests and progress of the Society (171).

Grant was one of the Fellows removed from the Publications Committee in April (1835) (172). At the same time the Tory junto recommended his and Gordon’s removal from the Council itself at the elections on 29 April. While the junto worked according to the letter of the law, they still had to invoke certain unwritten rules. The charter directed five members to be replaced annually, and the by-laws required the Officers to draw up a list (on the basis of attendance figures) to submit to the Fellows. If six or more Fellows dissented, they

could propose substitutes. The 29 April meeting, which took place at the Royal Institution, was noisy and ended in pandemonium, being adjourned until 27 May (173). What had happened was that *twenty* members objected to the removal of Grant, Gordon, and Capt. James Mangles, substituting the names of three Vice-Presidents, Sabine, Broderip, and Sir Robert Heron (174). This was justified by attendance figures, Broderip's (4) and Heron's (9) being less than Mangles' (10), Grant's (15) and Gordon's (19). But the reformers also wanted a combination of attendance and length of service to count hence Sabine was also on their hit list, being the longest serving Council member (elected 1826). Sabine managed to abort the meeting on a technicality – one Fellow having voted without having paid his subscription. This gave the junto time in which to publish a self-justifying *Statement*. In this they exempted office-bearers from democratic control (although there was nothing in the rule book about this) and announced that the Council had voted 11-4 not to substitute the Vice-Presidents. They retained control, being able to influence voting without themselves being subject to election. These were of course issues of democracy and accountability dear to reformers' hearts, and the junto's attempt to stave off much-needed management reform infuriated Wakley. He devoted the *Lancet* editorial on 23 May to the forthcoming meeting, employing the sort of radical invective for which he was famed:

a certain *clique* consisting of individuals who are notorious as well for their ignorance of the prin-

ciples and true objects of science as for their jobbing propensities, have had the impertinence, the brazen audacity, to propose ... that Professor GRANT and Mr. GORDON shall be two of the five gentlemen who ... are to be balloted out of the Council

... so scandalous is it, so thoroughly contemptible is it, so characteristic is it of besotted and grovelling stupidity on the part of its authors, that we could almost imagine that the ghosts of the Birmingham mob who beset and threatened the life of the philosopher PRIESTLEY, had infused a portion of their satanic spirit into the minds of the Fellows of the *Zoological Society*.

... in the sixth year of the Society's existence, it is proposed by a knot of jobbers that the only man amongst them who has a profound knowledge of the subject of zoology and animal physiology, should be excluded from the Council, – a proposition the barefaced insolence of which is only equalled by its black and daring iniquity, directed as it is against a man of spotless honour, who occupies a station in the field of comparative anatomy which no fellow labourer in this country approaches, and whose reputation as a cultivator of science has extended over the continent of Europe, throwing lustre in every quarter where his labours are known, on the scientific character of this country (175).

Wakley lambasted the junto's effort to swing the vote and curry favour, and deplored the use of proxy voting by lady fellows as unconstitutional. He finished

Had Professor GRANT confined his invaluable labours in the Society solely to the scientific departments of the institution, it is possible, just barely possible, that his presence might have been endured by the jobbers; but the honourable zeal of this really great man for the general interests of the Fellows, having led him, with other conscientious gentlemen, to take a part in the pecuniary management of the establishment, he has brought down upon himself the hatred and malignity of the entire band of mercenary speculators in the funds of the Society.

Events at this point become confused. The meeting on 27 May was rowdy and made the front page of *The Times*, but according

to *The Literary Gazette* Grant and Gordon were re-elected (176). The MS Minutes of the meeting, however, record a wholly different story – according to them the original five members were balloted out, including Grant (177). *The Lancet* in June seemed to confirm the newspaper reports, ecstatic that the junto had

been exposed and defeated; and, further, the perpetrators of the disgusting intrigue have been held up to universal detestation by a committee of the Society which has been appointed to inquire into the best manner of conducting the future elections of the officers, the members of which, on the 11th inst., passed a resolution in high commendation of the mode in which Dr. GRANT has exerted his abilities to promote the interests of the institution (178).

I do not know how to account for the discrepancy, or what intrigues took place *after* the 27th; the operative point, however, is that Grant, disgusted “by the offensive treatment of the Council” (179), declined all further contact with the Society. Others, too, threatened that if the junto won, they would consider themselves subject to arbitrary expulsion and, rather than submit to the indignity, would resign (180). Anyway, the upshot of the May events was that the hand of Tory amateurs like Broderip and anatomists like Owen was strengthened on the Council, making the Society at an official administrative level more representative of the anti-transformist, anti-radical feeling of its landed backers. Indeed, considering that at precisely this moment (20 May to be exact) Owen’s first pointedly, anti-Lamarckian paper was accepted by the Council for publication in the

Transactions (181), it is circumstantially likely that the conflict and purges contained an anti-transformist dimension.

Conclusion

Wakley's puffing was of course notorious and any attempt to exploit it uncritically to inflate Grant's social or scientific standing is to invite partisan distortion. Wakley's clear political sympathy for the Parisian-style zoologist is itself a caution. Certainly after 1833 there was never any lack of support from this radical quarter. The *Lancet* in 1836 echoed Turner's sentiment, calling Grant, "beyond all doubt, one of the most highly-gifted physiologists in Europe", and well-titled "The English CUVIER" (182). Invariably the circumstances of this extravagant praise explains all. Like many of the professors, Grant was deeply involved in the internecine feuds which periodically rocked the infant university. Turner's own statement in 1831 was made while *canvassing* for Grant – Turner was in fact petitioning James Mill in an effort to push Grant into Charles Bell's vacated Chair. Something similar was true of Wakley's motives. The seasoned political campaigner (he had taken Finsbury for the Radicals in 1835) was championing Grant when the Physiology Chair again became vacant on Jones Quain's retirement in 1836. Hence Wakley's superlative-laden rhetoric, his tactical exaggeration extolling Grant's "brilliant genius", and insisting that his

“acquirements in physiology...are not surpassed by those of any professor in Europe”.

(When the chair went to Sharpey, Wakley’s flailings became positively wild; he lashed the Council for its “hypocrisy, treachery, envy, and fraud, superadded to the one ancient evil, *love of self*”, exclaiming “the community, the profession, and the students of the institution, HAVE BEEN BETRAYED” (183)). Clearly these outbursts must be viewed in context – as political hyping and the rhetorical invective of a campaigner on the trail. There is no doubt either that similar ‘campaign-style’ distortion was in evidence in the ZS affair, which Wakley *qua* anti-monopolist obviously viewed in political terms. But having said all of that, I would nonetheless argue that it is precisely *because* of Wakley’s ideological sympathy that we get a profound insight into Grant’s social predicament. It was not solely that the University’s laissez faire arrangements bankrupted him and stopped his original output, or that he was actively discouraged from publishing. From a political perspective, Grant’s radicalism would have made him suspect in the eyes of conservatives – hence his brushes with the Peelites at the GS (Ch.7). This political perspective in the turbulent Reform years provides a unique insight into the underlying reasons for his loss of *institutional* power. He had, after all, switched almost exclusively to publishing in the ZS’s *Proceedings* and *Transactions* in the early 1830s; with this outlet effectively denied him, his sudden and sharp publishing decline from 1835 is far easier to explain. These essential social, political, and ideological factors must

figure prominently in any integrated explanation of Grant's decline after his first flurry of success. We must look to underlying *cultural* causes and take into account the methodological and managerial split in the zoological community – only then will we be able to understand political necessity of a strong social – and scientific – response by institutional managers to the radical threat. And, finally, it is only against this *broader* cultural framework that we can fully interpret the community response to Grant's Lamarckism. To complete the picture, I will now turn to a study of Grant's transformist lectures and their ideological base, while in Ch. 5 I will look more closely at the social and scientific response.

Notes and References

1. Geoffroy was still being dispassionately reported after Grant left, e.g. “Of the Continuity of the Animal Kingdom by means of Generation, from the first Ages of the World to the present Times”, *ENPJ*, 7 (1829), 152-5.
2. Roy Porter, “Gentlemen and Geology: The Emergence of a Scientific Career, 1660-1920”, *Hist. J.*, 4 (1978), 809-36.
3. H. Hale Bellot, *University College London 1826-1926* (University of London Press), 1920. My account is based on chs. 2-6, esp. pp. 29-33, 47-51, 79-80.
4. *The Lancet*, 2 (1830-1), 689-90.
5. Bellot (op. cit.3), 33.
6. *Medical Gazette*, 13 (1833), 49.
7. *Ibid.*, 7 (1830), 305.
8. Sir Alexander Grant, *The Story of the University of Edinburgh during its first three hundred years* (London, Longmans, 1884), i, 325. J. B. Morrell, “Science and Scottish University Reform: Edinburgh in 1826”, *Brit. J. Hist. Sci.*, 6 (1972), 39-56. Also idem., “Individualism and the Structure of British Science in 1830” *Hist. Stud. Phys. Sci.*, 3 (1971), 183-204.
9. S. T. Coleridge, *On the Constitution of the Church and State according to the Idea of each* (London, Dent, 1972; ed. J. Barrell), 53.
10. Figures calculated from “Professors’ Fees Book” MS, UCL. Cf. Bellot, 176, whose account does not tally with mine: according to him the College took only one-third.
11. Mrs. Lyell, *Life Letters and Journals of Sir Charles Lyell, Bart.* (London, Murray, 1881), i, 178; Bellot, op. cit. (3), 136-8.
12. College Correspondence Applications, UCL.
13. J. E. Bicheno, *An Address delivered at the Anniversary of the Zoological Club ... November 29, 1826* (London, Taylor, 1826), 18.
14. Managers’ Minutes, vii, f. 328, MS Royal Institution Archives.
15. *Zoological Journal*, 2 (1826), 568.

16. Managers' Minutes, vii, ff. 24, 62, 83, 85, 162, 164, 176, 209, MS Royal Institution Archives.
17. *DNB*.
18. *Zoological Journal*, 3 (1827), 309-10.
19. *Zoological Journal*, 4 (1829), 506.
20. J. Brookes to G. Birkbeck, 11 March 1826, UCL CC 1826:53.
21. L. Horner to J. Thomson, 7 July 1827, UCL CC 445.
22. J. Thomson to L. Horner, 9 July 1827, UCL CC 445.
23. Bellot, op. cit. (3), 38-9, 147-8.
24. *Statement by the Council of the University of London* (London, Longmans, 1827), 10-11. Cardwell also comments on this point: D. S. L. Cardwell, *The Organization of Science in England* (London, Heinemann, 1972), 46.
25. Compiled from *University of London: Medical Classes*, Records Office UCL.
26. R. E. Grant to L. Horner, 29 January 1830, UCL CC P142.
27. R. E. Grant to L. Horner, 8 April 1829, UCL CC P147.
28. UCL CC P128.
29. R. E. Grant to L. Horner, 5 November 1830, UCL CC P130.
30. R. E. Grant to L. Horner, 29 January 1830, UCL CC P142.
31. R. E. Grant to L. Horner, 18 November 1828, UCL CC P148.
32. R. E. Grant to L. Horner, 16 May 1829, UCL CC P147.
33. R. E. Grant to L. Horner, 25 January 1830, UCL CC P143.
34. R. E. Grant to L. Horner, 8 April 1829, UCL CC P147.
35. R. E. Grant to L. Horner, 8 April 1830, UCL CC 139; 24 April 1830, UCL CC P145.
36. R. E. Grant to T. Coates, 21 September 1831, UCL CC 2212.
37. R. E. Grant to L. Horner, 29 January 1830, UCL CC P142; also 29 May 1830, UCL CC P137.
38. *Distribution of the Prizes*, UCL College Collection, A 3.2. Bellot, op. cit. (3), 197, 199, 204. Eis dell was helping Grant as late as 1834, e.g. transporting

specimens to the Belgrave Institution in Sloane Street where Grant was lecturing – see Grant to T. Coates, 25 July 1834, UCL CC 3253.

39. R. E. Grant to L. Horner, 20 December 1828, UCL CC P149. He was adamant on the need to expose students to dissections: “Lectures”, *The Lancet*, 1 (1833-4), 93.
40. R. E. Grant to L. Horner, 20 June 1828, UCL CC 759.
41. R. E. Grant to L. Horner, 5 July 1828, UCL CC 760.
42. R. E. Grant to T. Coates, 3 July 1832, UCL CC 2567.
43. R. E. Grant to L. Horner, 18 November 1828, UCL CC P148.
44. R. E. Grant to L. Horner, 20 December 1828, UCL CC P149.
45. R. E. Grant to L. Horner, 20 October 1830, UCL CC P132; 29 May 1830, UCL CC P136.
46. R. E. Grant to L. Horner 28 May 1830, UCL CC P138.
47. G. Mantell to W. Clift, 22 July 1828, RCS Stone Collection, Autograph Letters. Mantell was trying to persuade Clift not to bid against him for iguana specimens he desperately wanted.
48. R. E. Grant to L. Horner, 22 July 1828, UCL CC P148.
49. These are mentioned or itemized in UCL CC 760, 759, P139, P140, P141, P148.
50. J. Brookes to President, 11 October 1830, UCL CC 1830: 1868. Also 21 October 1830, UCL CC 1830: 1872.
51. R. E. Grant to L. Horner, 20 June 1828, UCL CC 759; Grant to T. Coates, 30 January 1830, UCL CC 1715.
52. R. E. Grant to L. Horner, 1 November 1828, UCL CC P150.
53. R. E. Grant to C. C. Atkinson, 19 April 1837, UCL CC 3968. Hobson attended Grant’s classes in 1836-8 (“Professors’ Fees Books” MS UCL). For more on Hobson’s shipments to London see my Ch. 5.
54. “London University – Mr. Bell”, *Medical Gazette*, 7 (1830-1), 305-11. C. Bell, *Letters of Sir Charles Bell* (London, Murray, 1870). 316.
55. *The Lancet*, 2 (1830-1), 693.
56. Bellot, op. cit. (3), 195 *et seq.*
57. *Medical Gazette*, 7 (1830-1), 117-8.

58. *The Lancet*, 2 (1830-1), 744-50 (749).

59. Ibid., 763.

60. Ibid., 1 (1831-2), 56, 82, 84-5.

61. R. E. Grant to L. Horner, 21 September 1827, UCL CC Applications: Chemistry.

62. E. Turner to James Mill, 20 April 1831, reproduced in *The Lancet*, 2 (1835-6), 344.

63. *The Lancet*, 1 (1831-2), 188-9.

64. Ibid., 199-200. Grant's library no longer contains this book by Hercule Straus-Durckheim (1790-1865).

65. It was confidently expected that such a course would be run; e.g. a 36 week course on "Geology & Mineralogy" was advertised in 1828 (UCL CC 1179.11) on the assumption that a professor would be appointed. Yearly in fact the Council advertised the vacancy for a professor of geology and announced a proposed course in anticipation of his arrival. See *University of London: Annual Reports 1827-1834*.

66. Quoted in J. M. Edmonds, "The First Geological Lecture Course at the University of London, 1831", *Ann. Sci.*, 32 (1975), 257-75 (260).

67. Ibid., 261.

68. Ibid., 273; on the pecuniary saving, Bellot, op. cit. (3), 212-3; Bell talked of Horner running scared in *Letters*, op. cit. (54), 317-8.

69. Minutes of Council, Geological Society of London, 3 (1829-34), ff. 38, 155.

70. See the 15 autograph letters from Lindley to Hutton dated 1829-32 relating to their Fossil Flora (3 vols. 1831-7): BM(NH) Botany Library B. MSS Lin.

71. R. E. Grant to C. C. Atkinson, 18 February 1836, UCL CC 3609.

72. R. E. Grant to C. C. Atkinson, 1 April 1837, UCL CC 3592.

73. R. E. Grant, *General View of the Characters and the Distribution of Extinct Animals* (London, Bailliere, 1839).

74. [D. Brewster], "Decline of Science in England", *Quarterly Review*, 43 (1830), 305-42 (326).

75. J. F. C. Harrison, *Early Victorian Britain 1832-51* (London, Fontana, 1979), 131. The situation was not materially changed in the 1850s, when a junior clerk might earn £150-£300: G. Best, *Mid-Victorian Britain 1851-70* (London, Fontana, 1979), 107-9. Kitson Clark considers £60 to £150 or £200 the range for the lower middle classes in the second quarter of the century: G. Kitson Clark, *The Making of Victorian England* (London, Methuen, 1962), 119.

76. J. Russell, "The Late Professor Grant", *Medical Times and Gazette*, 2 (1874), 563-4.

77. Lyell, op. cit. (11), i, 161.

78. D. E. Allen, *The Naturalist in Britain. A Social History* (Penguin, Harmondsworth, 1978), 84; Jack Morrell and Arnold Thackray discuss relative earnings in *Gentlemen of Science* (Oxford, Clarendon Press, 1981), 309 n.56.

79. A. Desmond, *Archetypes and Ancestors: Palaeontology in Victorian London 1850-1875* (London, Blond & Briggs, 1982), Ch. 1.

80. Dr. Wilks and Dr. Daldy, *A Collection of the Published Writings of the late Thomas Addison, M.D.* (London, New Sydenham Society, 1868), ix-xviii.

81. C. Hall, *Memoirs of Marshall Hall* (London, Bentley, 1861), 69, 120.

82. B. C. Brodie, *Autobiography of the late Benjamin C. Brodie, Bart.* (London, Longman, 1865), 63-4, 116, 140.

83. Managers' Minutes, vii, ff. 85, 162, 164, 176, 209; Royal Institution Archives.

84. Lyell, op. cit. (11), i, 397.

85. *Zoological Journal*, 2 (1826), 569.

86. MS n.d. [prob. 1830], RCS 275 (18) h7; see also Rev. R. Owen, *The Life of Richard Owen* (London, Murray, 1894), i, 33, 61, 68.

87. R. Owen to Chairman and Board of Curators, 21 March 1833, RCS 275 (u) h7.

88. MS, 13 December 1838, RCS Misc. 2; cf. Owen Life, op. cit. (86), i, 68, for Clift's gratuity.

89. E.g. by T. H. Huxley: L. Huxley (ed.), *Life and Letters of Thomas Henry Huxley* (London, Macmillan, 1900), i, 74.

90. Lyell, op. cit. (11), is 397.

91. Morrell, “Science and Scottish University Reform”, op-cit. (8), 49; J. H. Ashworth, “Charles Darwin as a Student in Edinburgh, 1825-1827”, *Proc. Roy. Soc. Edinb.*, 55 (1935), 97-113 (100).

92. Bellot, op. cit. (3), 175.

93. Ibid., 179.

94. Data from “Professors’ Fees Book” MS UCL.

95. [Brewster], op. cit. (74), 326.

96. R. E. Grant to L. Horner, 12 March 1831, UCL CC 2397.

97. BS, 690.

98. R. E. Grant to C. C. Atkinson, 3 December 1842, UCL.

99. *The Lancet*, 2 (1850), 711.

100. R. E. Grant to C. C. Atkinson, 22 September 1837, UCL CC 4166.

101. *The Lancet*, 1 (1836-7), 21.

102. R. E. Grant to W. Hutton, 14 December 1838, American Philosophical Society, B:H978, William Hutton Papers.

103. Russell, op. cit. (76), 563-4.

104. J. B. Morrell, “London institutions and Lyell’s Career: 1820-41”, *Brit. J. Hist. Sci.*, 9 (1976), 132-46 (132).

105. M. Berman, *Social Change and Scientific Organization. The Royal Institution, 1799-1844* (London, Heinemann, 1978), xxi

106. J. N. Hays, “Science in the City: The London Institutions 1819-40”, *Brit. J. Hist. Sci.*, 7 (1974), 146-62 (147).

107. *An Historical Account of the London Institution* (London, 1835), Guildhall Library Pamphlet 466, pp. 28, 36.

108. Ibid. p 35; Hays, op. cit. (106), 151.

109. London Institution Handbill (1835) of Grant’s Syllabus of a course of eight lectures on invertebrates: Guildhall Library.

110. BS, 691; see Grant’s letter to Faraday, 13 January 1837, Faraday Folio 11, f. 135, Royal Institution

Archives.

111. Berman, op. cit. (105), 126.
112. Data on attendance of Grant's Friday Evening Lectures: Royal Institution Archives. For reports see *Medical Gazette*, 12 (1832-3), 479; 13 (1833-4), 927-8; 14 (1833-4), 425-6; 17 (1835-6), 831-2; 19 (1836-7), 749-50; 23 (1838-9), 840-1; 24 (1838-9), 58-60.
113. For this he received £42: Managers' Minutes 1832-53, Vol. VIII, f. 203, Royal Institution Archives.
114. Ibid., ff. 498, 514, 549, 552.
115. Ibid., Vol IX, f. 15
116. J. Barlow to R. Owen, n.d., BM(NH) OC Vol. 2, f. 220; Managers' Minutes, Vol XI, f. 149, Royal Institution Archives.
117. Rev. Owen, op. cit. (86), i, 80.
118. See Philip to Richard Taylor (his brother), 9 August 1830, Taylor Papers, St. Brides Printing Library, 11, discussing the working men's role in the July Revolution.
119. The other backers were Turner, Clift, Mayo, and the obstetrician Robert Lee (1797-1877): *Certificates 1830-1840*, VIII, 182, Royal Society. Grant had recommended two of his backers (Turner and Elliotson) for their London Chairs. Lee, Bostock, Elliotson, and Turner were all Edinburgh educated. During the 1820s and 1830s, even before the 1839 reforms, greater recognition was being given to science over status: M. Crosland, "Explicit Qualifications as a Criterion for Membership of the Royal Society: A Historical Review", *Notes and Records of the Royal Society*, 37 (1983), 167-87 (179-83).
120. He did referee Mantell's belemnite paper in 1850 and Bowerbank's sponge paper in 1861: *Reference Reports 1850-55*, RR.2, f. 146; and *1863-5*, RR.51 I. 26.
121. R. E. Grant to T. Bell, 5 March 1849, *The Lancet*, I (1850), 88 – see Wakley's accompanying editorial.
122. Ordinary Minute Book 5, 1830-1832, f. 119, Geological Society of London.
123. W. Swainson, *A Preliminary Discourse on the Study of Natural History* (London, Longman, 1834).
124. Council Minute Book No. 2 1826-43, ff. 61, 82-3,

Linnean Society Library.

125. He was re-elected to the Council in 1859 for a year: Council Minute Book No. 4 1859-72, ff. 6, 36, Linnean Society Library.

126. See Ch.1, n.150.

127. Morrell, op. cit. (104), 139.

128. Darwin's belief in evolution accompanied neuroses of personal betrayal. Owning up to it was like 'confessing a murder', and he sought absolution by taking the earliest opportunity of sending copies of the *Origin* to Sedgwick et. al. F. Darwin (ed.), *The Life and Letters of Charles Darwin* (London, Murray, 1887), ii, 23.

129. Ordinary Minute Book No. 4 1828-1830, f. 371; No. 5 1830-1832, f. 331, Geological Society of London.

130. Morrell & Thackray, op. cit. (78).

131. Minutes of Council, Vol III, f. 17, Zoological Society.

132. Ibid., f. 290.

133. Ibid., Vol 1, f. 449; H. Scherren, *The Zoological Society of London: A Sketch of its Foundation and Development, and the Story of its Farm, Museum, Gardens, Menagerie and Library* (London, Cassell, 1905), 47. *Proceedings of the Committee of Science and Correspondence*, Part 1 (1830-1), 1-2.

134. Minutes, op. cit. (131), Vol 111, ff. 107, 354, 436-7; on the proposed new museum – *Zoological Society Reports Etc 1829-1850*, "Report 1834", pp. 13-190 ZS Library.

135. J. Brookes, *An Address delivered at the Anniversary Meeting of the Zoological Club ... November 29, 1828* (London, Taylor, 1828), 27.

136. Cf. Bicheno's earlier and fuller listing of periodicals dealing with natural history, op. cit. (13), 21.

137. [W. Broderip], "The Zoological Gardens – Regent's Park", *Quarterly Review*, 56 (1836), 309-332 (331).

138. W. Kirby, "Introductory Address", *Zoological Journal*, 2 (1825), 1-8 (5). On the circulars sent via the Colonial Office: P. Chalmers Mitchell, *Centenary History of the Zoological Society of London* (London, ZS, 1929), 32-3. Colonial imports were received from the Himalayas, Nepal, Cuba, Central Africa, Australia, etc.: Bicheno, op. cit. (13), 5-9.

139. Despite pressure in the 1820s and 1830s from individual reformists/declinists (e.g. Bicheno, Brookes, Grant, and Swainson) for state support, this kind of ‘commodity’ approach was not successful until the Huxley/Tyndall/Lankester camp exploited it in the 1860s-1870s by playing on fears of Britain’s industrial decline following the disastrous Paris Exhibition of 1867: Desmond, *Archetypes and Ancestors*, op. cit. (79), Ch. 3. On Grant’s complaint that comparative anatomy was dismissed as “merely” a branch of natural history in Great Britain and “thus almost derived of public support”, see *The Lancet* 1 (1833-4), 97.

140. Porter has commented on Lyell’s dislike of French professional bureaucratic science. Notice too that Darwin preferred the English *geological* to zoological communities: this says a good deal about their social differentiation: Porter (op. cit.2), 823.

141. As a bare minimum this core group would include T. Bell (1792-1880), E. T. Bennett (1797-1836), J. E. Bicheno (1785-1851), J. Brookes (1761-1833), J. G. Children (1777-1852), J. E. Gray (1800-1875), T. Horsfield (1773-1859), W. Kirby (1759-1850), W. S. Macleay (1792-1865), J. Sabine (1770-1837), J. F. Stephens (1792-1852), G. B. Sowerby (1788-1854), J. de C. Sowerby (1787-1871), N. A. Vigors (1785-1840) and W. Yarrell (1784-1856).

142. For modern studies of zoologists holding public office see particularly A. E. Gunther’s works, including *The Founders of Science at the British Museum 1753-1900* (Suffolk, Halesworth Press, 1980), Chs. 5-7; “John George Children”, *Bull. Brit. Mus. (Nat. Hist.). Historical Series*, 6 (1978), 75-108; “The Miscellaneous Autobiographical Manuscripts of J. E. Gray”, *ibid.*, 6 (1980), 199-244; J. Bastin & D. T. Moore, “The Geological Researches of Dr Thomas Horsfield in Indonesia 1801-1819”, *ibid.*, 10 (1982), 75-115. But especially see the excellent article by S. Sheets-Pyenson, “Geological Communication in the Nineteenth Century: the Ellen S. Woodward Autograph Collection at McGill University”, *ibid.* 10 (1982), 179-226.

143. See “Introduction”, *Zoological Journal*, 1 (1824), iii-vii, and the yearly addresses; also S. Herbert, “The Place of Man in the Development of Darwin’s Theory of Transmutation. Part II”, *J. Hist.. Biol.*, 10 (1977), 155-227 (170-6).

144. Kirby, op. cit. (138), 5.

145. Bicheno, op. cit. (13), 22-5. On a broader plane, these were not strictly cries in the wilderness; state intervention did grow steadily in many sectors after the Reform Bill, despite a dominant individualist ethos.

This historiographic point is made by G. B. A. M. Finlayson, *England in the Eighteen Thirties* (London, Arnold, 1969), 65 *et seq.*

146. Others, like Brookes, did however welcome signs of government patronage, Brookes, op. cit. (135), 26.
147. Swainson, op. cit. (123), 324, 353, 362-5, 387 *et seq.*
148. J. G. Children, *An Address delivered at the Anniversary Meeting of the Zoological Club ... November 29, 1827* (London, Taylor, 1827), 11.
149. Bicheno, op. cit. (13), 27-30.
150. Kirby, op. cit. (138), 7-8; Swainson, op. cit. (123), 83-8.
151. W. Kirby, *On the Power Wisdom and Goodness of God as Manifested in the Creation of Animals and in their History Habits and Instincts* (London, Pickering, 1835), i: xxiv, xxvii.
152. E.g. *Trans. Linn. Soc.*, 14 (1823-5), 46; *ibid.*, 395; 15 (1826-8), 479; 16 (1833), 1; *Zoological Journal*, 4 (1827), 43; *ibid.*, 401; *Mag. Nat. Hist.*, 9 (1836); 4 (1840) 141, 305; *Ann. & Mag. Nat. Hist.*, 6 (1841) 184; 7 (1841), 41; 9 (1842), 197. Cf. *Phil. Mag.*, 62 (1823), 192, 255; 6 (1829), 199; 8 (1830), 52, 53, 134, 200, for Fleming vs. Macleay.
153. Kirby, op. cit. (138), 5-6; but cf. Kirby's letter to Fleming in J. Fleming, *The Lithology of Edinburgh* (Edinburgh, Kennedy, 1859), 46-7; Brookes (op. cit. 135), 5; N. A. Vigors, "Observations on the natural affinities that connect the orders and families of birds", *Trans. Linn. Soc.*, 14 (1825), 395-517.
154. Fleming, *ibid.*, 73, also 45-6, 37, where he describes a visit to the Zoological Club: "Nothing is the *go* here but quinary divisions, and groups returning into themselves." For his opposition see [J. Fleming], "Systems and Methods in Natural History", *Quarterly Review*, 41 (1829), 302-27.
155. J. O. French, "An Inquiry, respecting the term Nature of Instinct, and of the mental distinction between brute animals and man", *Zoological Journal*, 1 (1824), 1-32, 153-73, 346-67.
156. *Zoological Journal*, 2 (1825), 428.
157. *Ibid.*, 424-5.
158. Herbert, op. cit. (143), 159-64; M. J. S. Rudwick, "Charles Darwin in London: The Integration of Public and

Private Science”, *Isis*, 73 (1982), 186-206 (203-4).

159. Brookes, op. cit. (135), 27; J. Bastin, “The First Prospectus of the Zoological Society of London: New Light on the Society’s Origins”, *J. Biblio. nat. Hist.*, 5 (1970), 369-88. This and Bastin’s follow-up paper are extremely important: J. Bastin, “A Further Note on the Origins of the Zoological Society of London”, *ibid.*, 6 (1973), 236-41.

160. Harrison, op. cit. (75), 119.

161. Quoted in Bastin, “First Prospectus”, op. cit. (159), 370.

162. Quoted in D. P. Miller, “Between Hostile Camps: Sir Humphry Davy’s Presidency of the Royal Society of London, 1820-1827”, *Brit. J. Hist. Sci.*, 16 (1983), 1-47 (36-7).

163. “More than fifty years have passed, and British-grown armadillo has not yet appeared upon the menu-cards of our dinner table”, announced a later President: W. H. Flower, “The Zoological Society of London”, in *Essays on Museums* (London, Macmillan, 1898), 171-84 (175). The farm in Kingston was originally purchased (in 1829) to carry out experiments in domestication: *Zoological Journal*, 4 (1829), 522-3.

164. *Zoological Journal*, 3 (1827), 309-10.

165. Brookes, op. cit. (135), 28.

166. *Zoological Journal*, 4 (1829), 521.

167. *The Lancet*, 2 (1834-5) 199.

168. S. Squire Sprigge, *The Life and Times of Thomas Wakley* (London, Longmans, 1899), 304.

169. *The Lancet*, 2 (1834-5), 389.

170. Swainson, op. cit. (123), 305, 314-5, 439-41.

171. *The Lancet*, 2 (1834-5), 199.

172. Minutes of Meetings, Vol. 21 f. 7, Zoological Society.

173. *The Literary Gazette*, No. 954, 2 May 1835, 280.

174. *Statement by the President and Certain Members of the Council of The Zoological Society, in Reply to Observations and Charges made by Colonel Sykes and Others, at the General Meeting of the Society, on the 29th of April last, and at the Monthly Meeting on the 2nd of the same month* (London, Nicol, 1835), Appendix 13.

175. *The Lancet*, 2 (1834-5), 263, 199.
176. *The Times*, 29 May 1835, 1; *The Literary Gazette*, No. 958, 30 May 1835, 344.
177. Minutes of Meetings, Vol. 2, f. 18, Zoological Society.
178. *The Lancet*, 2 (1834-5), 390.
179. BS, 694.
180. Colonel Sykes quoted in *The Lancet*, 2 (1834-5), 200.
181. Minutes of Council, Vol. 4, f. 158, MS Zoological Society. R. Owen, "On the Osteology of the Chimpanzee and Orang Utan", *Trans. Zool. Soc.*, 1 (1835), 343-79.
182. *The Lancet*, 2 (1835-6); 676; ibid., 844; also 610-11, 646-8, 675-8; 1 (1836-7), 21. Grant did apply for Quain's Physiology Chair – see R. E. Grant to C. C. Atkinson, 8 August 1836, UCL CC 3715.
183. Ibid., 2 (1835-6), 789-91 (789).

Chapter 4

Grant's London Lectures and their Influence

Attended a lecture of Flourens – with Dr Grant – who said that if he had given one like it in 20 minutes his pupils would have put on their hats & walked off. On the structure & function of the Resp. Organs in Mammifi, Oiseaux, Reptiles – the most superficial & well known facts

Richard Owen, attending lectures at the Jardin des Plantes
with Grant in summer 1831 (1).

In the last chapter we saw how the *arrangement* of Grant's lectures was partly a response to the troubled events of 1831. We looked also at the causes of his financial embarrassment, and explained his publishing decline after 1835 as the result of a complex set of factors: pecuniary difficulty and consequent lecturing load, institutional discouragement, and loss of resources following his dispute with the ZS “junto”, itself reflecting the wider disagreements between radicals and conservatives in these volatile years.

Only in the 1830s could he be described as successful; indeed, for a time during the middle years of the decade he was considered the foremost comparative anatomist in the country. (Or, more accurately, his merits were appreciated variously by the different political factions – but since Whigs, middle-class reformers, and radicals were strongly placed after the reform of parliament, so Grant's reputation

peaked at this time.) The present chapter, then, paints a brighter picture. Here we discuss the structure of Grant's comparative anatomy lectures and their impact on the liberal wing of the profession – lectures that by common consensus were the most progressive, comprehensive, and “philosophical” ever to have been delivered in Britain. His entire course of sixty lectures was published in 1833-4. At the time he had just stepped down from the Council of the GS. He was beginning his association with the Royal Institution, and seemed impregnable at the ZS: sitting on three administrative committees and the Council, lecturing the Fellows, and submitting a string of papers to the *Proceedings*. The years 1832-5 arguably marked the height of his institutional power in London. Few of the problems had surfaced which were to mar the next few years (even if financial projections looked worrying). He was taking on commitments in diverse directions, many to remain unfulfilled. He was slated to write a tome on comparative anatomy for the Society for the Diffusion of Useful Knowledge in 1833, evidently to be based on his ZS course (2). Nothing came of it. Before 1835 he and Owen were named as the main editorial associates on R. B. Todd's *Cyclopaedia of Anatomy and Physiology* project (3). But Grant, after writing a well-received review of the “Animal Kingdom” for the second number (4), plus two short pieces, fell out with the fellow editors – ostensibly over failure to ensure proper acknowledgement, although quite what machinations were at root is difficult to tell (this being the time of the ZS incident). He still kept a number of irons

in the fire, and in 1833 looked set to become, if not the *Lancet*'s new Cuvier, at least institutionally secure and, in this reformist climate, scientifically respected.

Yet the Geoffroyan philosophy, covert transformism, and deistic context of these lectures must have alerted the conservative community. The fact, too, that they were published in the *Lancet* – and that he publicly praised the rabble-rousing Wakley in class – set them firmly into a *radical* context, making it difficult for them to be judged from any other perspective. The political implications I will leave until the next chapter. Here the *content* of the lectures is my prime concern, the extent of his application of Geoffroyan transcendentalism and transformism, and the *positive* influence of his science. For there is no doubt that these lectures were extremely influential in liberal quarters: as the first systematic philosophic exposition of comparative anatomy published in English to rival, say, Henri de Blainville's lectures in Paris (Cuvier being dead and Geoffroy having pretty much ceased lecturing). There had admittedly been other series run: J. H. Green's Coleridgean course at the College of Surgeons (1824-7) (see next Ch.), and J. F. South's course at St. Thomas's Hospital. But neither were published, and only reached exclusive audiences. The *Medical Gazette* had run a short course of ten comparative anatomy lectures by the Birmingham practitioner S. Langston Parker (1803-71) in 1830-1, but it concentrated on the

nervous system and was designed exclusively to illustrate human physiology (5). Grant's *sixty* lecture series, delivered each autumn at the University and published complete in 1833-4, reached a considerably larger audience than any of the foregoing. The influence of Grant's lectures on his students and auditors will be studied by targeting specific individuals. I have found that the best approach is to extend the work of the late Dov Ospovat, whose attempt to reconceptualize the old Huxleyan antithesis of Creation vs. Evolution led him to recategorize a number of biologists in the 1830s and 1840s as 'anti'-teleologists. I hope to show that, of the individuals he cites, some (P. M. Roget, W. B. Carpenter) were directly influenced by Grant and can now more accurately be seen as modifying his work. From this it will follow that Grant's influence in the 1830s might actually have been crucial to the shift away from Cuvierian functionalism towards a Geoffroyan structural approach. Thus I believe we can assign a distinct *historiographical* importance to Grant, quite apart from his role as a transformist whipping-boy. In future he must be seen as essential to any investigation of the movement away from Paleyite teleology to newer design arguments based on 'unity of plan'. This teleological-morphological shift in the 1830s has recently been the subject of study by historians (6). However, by bringing in Grant, I hope to show that the scientific shift had a heretofore undisclosed radical origin, and therefore that the episode can be related to contemporary political stances.

Comparative Anatomy Lectures 1833-4: Major Themes

Grant travelled yearly to Paris to attend lectures at the Jardin. He argued in 1830 that “if we wish to view the study of animated nature in a form truly worthy of occupying a philosophic mind, we must direct our attention to the French school” (7). And as a reformer and poorly-paid academic, he was not slow to point out that it was the “liberal aid afforded to science by the French government” which had allowed France to pull ahead of other European countries in this area. While he might not have thought much of the lectures of Cuvier’s protégé Pierre Flourens, he was impressed with the philosophic anatomy of Etienne Geoffroy St. Hilaire (1772-1844) and the animal series of Henri Marie Ducrotay de Blainville (1777-1850). These were the two most powerful anatomists at the Jardin after Cuvier’s death (in 1832); indeed, two who had publicly debated Cuvier while alive on methodological matters, and become caught up in invidious politicking at the Académie (Toby Appel has documented this in great detail (8)). Grant’s unswerving allegiance to Blainville and Geoffroy – strengthened if anything after Geoffroy’s clashes with Cuvier at the Académie in 1830 – was to have important ramifications at home.

Four unifying aspects of Grant’s lectures are immediately striking. These are:

1) Geoffroy's Unity of composition

[Comparative anatomy uncovers] to us unexpected analogies in the forms and structure of parts in animals remote from each other in the scale, and by extending those analogies it leads us to perceive a resemblance of structure in very different classes of animals, and a uniformity of system – a unity of plan – in the organization of the whole animal kingdom.

Grant opening his lectures in 1833 (9).

In chapter two we judged the relative success of Geoffroy's homological programme in *Philosophie Anatomique*, in which he identified the analogues of the opercular bones enumerated by Cuvier for fishes in the inner ear ossicles in mammals. His theory of analogies and principle of connections was widely taken up in the 1820s by Savigny, Latreille, and Audouin, and Geoffroy himself began his teratological experiments to determine the environmental influence on foetal development, hoping to generate malformations lower in the scale which might support his theory of unity of composition. But Cuvier's functionalist opposition became pronounced after 1825, and the threat of loss of patronage caused Latreille at least to abandon the search for homologies between the *embranchements*. The timing of Cuvier's opposition was politically opportune – his celebrated debate with Geoffroy at the conservative Académie coincided with the revolutionary upsurge preceding Charles X's abdication; and it is clear that the debate, while ostensibly about anatomical minutiae, had metaphysical and political overtones. Cuvier in his *Dictionnaire* article "Nature" in 1825 (10) rooted his

religious opposition in the belief that nature was incomprehensible without God. He tactically linked the chain of being, transformism, recapitulation, and unity of composition, as theories founded on the false premise of nature's autonomy, and thus to be rejected *in toto*. Geoffroy on the other hand was a deist who began his onslaught on vitalist explanations about the time that his hostility to Cuvier's functionalism became pronounced. Geoffroy's supporters included younger medical men (presumably more materialistically inclined than Cuvier's established backers – or with less to lose, in terms of patronage); and Appel notes that Geoffroy's views were attractive for political reasons: as a self-developing alternative to an autocratic religious universe in which change and reform were ruled out (11).

Grant characteristically accepted the four theories seen to usurp God's power: unity of composition, recapitulation, transformism (called by Grant and Geoffroy, "metamorphosis"), and the animal series. Indeed, Grant no less than his critics (Owen – see Ch. 7) probably accepted a certain logical relationship between the four. From Cuvier's accusations, too, it is obvious that this interrelated complex provided an ideal scientific expression of bourgeois reform ideology – as an anatomical justification of continual progress and change without destruction of the underlying social unity (by revolution), while the loose deistic base and delegation of Divine power provided a tool to break up the entrenched power

of the Established Church.

Geoffroy's philosophical anatomy formed the leit motif of Grant's lectures. Geoffroy, he wrote, "has surpassed all his predecessors and contemporaries, by his profound, philosophical, and original views of the anatomy of the higher animals, and by the ingenuity and boldness of his speculations in regard to the development, and the analogy of organs and general laws of the animal economy" (12). He also recognized that the results of E. de Serres' studies on recapitulationist ontogeny, and the development of the nervous system "form a striking confirmation of the philosophic views of GEOFFROY on the unity of Plan" (13). For Grant it wasn't simply that organisms adhered to type (an Owenian idea that became prominent in the 1840s). True, he noted the astonishing osteological similarity of the human hand, elephant's foot, mole's trowel, bat's wing, and fish's fin (14) – but he understood it in a much wider sense:

When we examine the internal mechanism of animals throughout the whole of the divisions of a class, we are struck with the similarity of the general plan upon which they have been constructed in all their apparatus for self-preservation, and the continuance of the race. By comparing the organization of one class with that of another belonging to the same great subkingdom, we perceive only slight modifications of the same plan; and by continuing the comparisons, and extending them through all the great divisions of the animal kingdom, from the highest to the lowest, we observe the gradual disappearance of whole systems of organs, but in those which remain we distinctly, perceive the rudiments of the same plan of formation (15)

The course was thus a vigorous attempt, not merely to work

out Geoffroy's programme, but to extend it to the entire animal series – to point out, e.g., that in fact many organs like the liver do *not* disappear on descending the chain, but can be recognized in monas, hydra, etc, as follicular granules (16) – and that in truth almost all organisms display a characteristic unity of composition whatever their rank in the series. This was a bold conception, and it is important to emphasize in order to understand the manoeuvres of his protagonists. Grant was consciously occupied in “reducing apparent exceptions to the general laws of development, and in demonstrating a unity of plan throughout all the grades of animal organization” (17).

On osteological matters, he adhered to Geoffroy's “philosophic nomenclature” (18), giving detailed accounts of the analogical relationships of the hyoidean, sternal, and vertebral elements (19). He followed Geoffroy closely on specifics, such as relating the piscine opercular bones to the inner ear ossicles of mammals (20). Grant was never one to extol Cuvierian functionalism, and he warned against explaining apparently anomalous bones teleologically without at first examining rival possibilities – countering Cuvier's (and Owen's) teleological explanation of the opercular plates in fishes as *adaptive* features (21). He gently chided the “illustrious” Cuvier, who had lodged “powerful objections” to the “pretended unity of composition”, insisting that they could be overcome and the “extraordinary” unity preserved “by

exceedingly minute and careful examination” of the ontogenetic stages (22). He also came out clearly against any model composed of discrete *embranchements*.

Practically, in a taxonomic sense, he did use Cuvier’s fourfold division – Vertebrata, Mollusca, Articulata, and Radiata – admitting:

They are founded on extensive and accurate analogies, and have been sanctioned by long and general adoption; but they convey no idea of any uniform principle of classification applied to the whole animal kingdom (23).

Each had been defined differently: the first by its vertebral column, the second by its softness, the third by its exoskeletal articulations, and the radiates by “a peculiarity in the external form of the body almost limited to the echinodermata, and not applicable to the entozoa, zoophyta, and infusoria”.

We cannot help thinking [he wrote in his review Baron Cuvier] that the science of comparative anatomy is now so far advanced, as to afford the means of distributing the animal kingdom on some more uniform and philosophic principles, – as on the modifications of those systems or functions which are most general in the animal economy. The characters for such a philosophical distribution might be looked for in the modifications of the generative system, or the digestive, or the nervous system It is greatly to be regretted that, with his vast resources for the improvement of this part of our nomenclature and arrangement, he has overlooked these minutiae of zoological distribution and remained fettered by his earliest views of classification.

He therefore *redefined* the divisions according to uniform nervous criteria, renaming vertebrates “Spini-Cerebrata”, molluscs “Cyclo-Gangliata”, articulates “Diplo-Neura”, and

radiates “Cyclo-Neura” from the characteristic ganglion arrangements (24). His goal was to distribute the animal kingdom on “more uniform and philosophic principles”, and to give the series from monad to man a common theme.

2) The Continuity of the Animal Series

Nature knows no sudden transitions. Our ignorance, however, often makes us think that she passes rapidly from one form to another, and that there is thus a sudden transition, – a break, a gap, in Nature’s works (25).

The idea of a single criterion allowing a uniform spread from monad to man was supported by Grant’s belief in the absolute continuity of the animal series and the inexorable progression from simple to complex – in historical development, the living chain, and ontogenetic growth which recapitulated the former two. This taxonomic/temporal extrapolation of the same pattern or plan was alluded to throughout his lectures, allowing him to swing freely between comparative anatomy and zoological history. He put this graphically:

When we speak of animals low in the scale, it is equivalent to our speaking of animal forms that have existed in the primitive conditions of this planet; for everything shows, that this kingdom itself has had a development from the most simple forms, and that in the first condition very likely nothing existed but myriads of animalcules swimming in the heated ocean that encompassed this cooling planet (26).

An identical profession of faith in zoological development is

to be found in these lectures, the *General View of the Character and the Distribution of Extinct Animals* (1839), and in the Swineyan course on “Palaeozoology” (1853), suggesting that his thought failed to undergo any radical change through three decades. Typically he argued that

Nature begins by simple forms. The animal kingdom itself began by the most simple forms, as is attested by what is found in the earth. Gradually it became more complex. That is attested alike by sacred and profane evidence. It accords with the sacred writings; it agrees with all the best facts drawn from the strata of the earth. Those beings which occur near the surface are of more complex structure, and man, whose remains have yet only been found in the newest strata, is the most complex of all (27).

Empirically this might not always appear the case, but if the oldest-known rocks did not contain the simplest animalcules it was only because the strata housing the primordial infusoria and zoophyta had been metamorphosed by heat and pressure into “crystallised limestone” and lost forever (213). (This *ad hoc* device was to find increasing use in his later palaeozoology lectures.)

In these 1833 lectures he concentrated on the two-fold progression: from monad to man within the series, and its ontogenetic recapitulation. For the idea of the series he was indebted to Blainville, whose lectures he first attended at the Jardin in late 1815, and whom he continued to visit until 1850. In 1833 Blainville had just been appointed Cuvier’s successor at the Jardin. He displays, Grant said, “an extensive and minute acquaintance with the organization of

the animal kingdom, and his materials are disposed in a luminous and philosophic arrangement” (29). Toby Appel has studied Blainville’s personal opposition to Cuvier and tactical adoption of Lamarck’s rival series. In the 1830s Blainville was the premier zoologist in Paris, and Appel believes that his animal series was better supported than is generally believed. Blainville had finally broken with Cuvier in 1816 (about the time Grant first attended his lectures); for the next few years he had concentrated on molluscs and zoophytes, possibly angling for Lamarck’s Chair. Despite his profound ideological differences with Lamarck – his dislike of transformism, spontaneous generation, and especially, his deistic materialism – the Catholic Blainville championed the Lamarckian chain as an alternative to Cuvier’s *embranchements*. Thus in 1816 he denied any divisional discontinuity by insisting that the segments of vertebrates (“internal Osteozoa”) were homologous to those in articulates (“external Osteozoa”). He sought to interpose linking groups between articulates and molluscs, and articulates and radiates, giving the sort of continuity flatly denied by Cuvier. Blainville’s philosophical anatomy differed only on specifics from Geoffroy’s – he believed, e.g., that the lower jaw elements of land vertebrates were the analogues of the opercular bones. Poor personal relations might have existed between fellow Catholics Geoffroy and Blainville, and the former might later repudiate many of his early beliefs (unity, a staggered creation, passage between types), but in

the 1820s, as Appel writes, “The younger generation of naturalists studying at the Faculty of Sciences were exposed to a double dose of philosophical anatomy from the two professors of zoology” (30).

Grant’s general approach was therefore typical of the anti-Cuvierian opposition in Paris. He talked incessantly of “imperceptible gradations” and taught that should nature appear “suddenly to bound forward” we can always “by a careful and extended comparison … trace the successive steps of the same regular and uniform plan of development which she has undeviatingly followed in the perfecting of individual systems of the economy, and in the construction of all organic forms” (31). He exemplified the smooth gradations between classes. Thus, while lecturing on amphibians, he used *Menobranchus*, *Proteus*, and the axolotl to illustrate the transition from water to land living:

Those animals breath by gills, as you see in this axolotl that I hold in my hand, which have the bronchial arches largely developed, and hanging externally from the sides of the neck, so that this is a tadpole without metamorphosis. The transition, then, from the water-breathing to the air-breathing vertebrate is gradual and almost imperceptible, from fishes to the amphibia, in many of which the branchiae are entirely lost (32).

Homological studies likewise highlighted the smooth passage from fish to reptiles, and reptiles to mammals; but the “march of development” did not stop there: it extended to man himself.

All transitions in organic forms are effected by imperceptible gradations, and according to determinate and uniform laws. This is not more obvious in the separate systems of internal organization than in the outward shape of the entire aggregates. So that there can be no organs developed in the semi-erect climbing *quadrumanous* animals, or in the erect *bimana*, which have not passed through their phases of development in the inferior tribes. Without galagos and loris and lemurs, the organization of the higher quadrupeds would be as anomalous as that of man would appear, without gibbons and orangs, and chimpanzees and negros (33).

To preserve absolute continuity throughout the series, he not only traced a gradual ascent within *embranchements*, but like his Parisian counterparts looked for connecting links *between* them. It was pointless his establishing universal criteria which would allow an anti-Cuvierian “uniform” taxonomic distribution from monad to man if he could not overcome objections based on the more apparent structural gaps. To get from radiates to articulates, for example he extended the graduated series first mooted in 1826:

We pass by beautiful gradations, from the horny *porifera* through the soft *alcyonia*, where the pores are developed into polypi, through the various flexible and solid zoophytes, to the complex corticiferous species, as the *isis*, which leads us to the fixed ramified and jointed family of crinoidea among the echinoderma (34).

In the next lecture he could then proceed to the bottom of the articulate division:

By the lengthening of the axis of the globular *echinida*, and the softening of the exterior shell, nature has arrived at the forms of the various holothuriae, and these lengthened, cylindrical, and soft echinoderms lead us naturally to the worms at the bottom of the articulated division of the animal kingdom (35).

But after the celebrated clash at the Académie over the analogical relations of molluscs and Geoffroy's account of the affair in *Principes de Philosophie Zoologique* (1830), Grant's most dramatic bridge was built between molluscs and vertebrates. Cephalopods had always been his speciality. He had dissected them at Edinburgh, and his study of *Sepiola* and *Loligopsis* at the Bruton Street museum tended to emphasize their proto-piscine structure. A new *Loligopsis* from the Indian Ocean connected the simple gasteropods and pteropods "with the more elevated forms of *Vertebrata*", he noted in the ZS's *Proceedings* (36), and the crescentic cartilaginous plates supporting *Sepiola*'s fin bore "a singular resemblance in their mode of attachment to the anterior extremities of *Vertebrata*" (37). The branchiae were intermediate between those of Pectinibranchiate gasteropods and cyclostome fishes. But these speculations at the ZS were only pointers to the fully-developed views expressed in his course. In consecutive lectures on cephalopods and cartilaginous fishes he traced the "successive degradation" of the invertebrate shell from the snail through the *Sepiola* to its "remnant" state in the armour of lowly sturgeon-like fishes (38), at the same time as singling out the emergence of diagnostic vertebrate traits in the molluscs:

As we find, in the class of fishes, remains of the external shells, in the form of calcareous scales, or plates, or solid spines, so we find in the cephalopods the first soft cartilaginous rudiments of the vertebral column, which lead us gradually to the still-imperfect condition of that central part

of the osseous system which is met with in the *myxene*, the *lampreys*, and others of the lowest cartilaginous fishes (39).

Because Grant *expected* structural continuity he had no trouble interpreting the neck cartilages of *Sepiola* as the “first rudiments of the cranial vertebrae, [and] of the rest of the vertebral column, although not yet divided into distinct vertebrae” (40). He argued that cephalopod fins with their supporting laminae were incipient fish pectorals, and that the oesophageal ganglia of the highest molluscs “already, approximated in form, position, and texture, to the brain of the fishes”; indeed, that “all the great systems of nerves of the vertebrate were already developed in the cephalopods” (41). Thus nature was moulded into a great progressive chain, and Grant, like Geoffroy and Blainville, could point out connections of extraordinary detail between Cuvier’s supposedly discrete *embranchements*.

3) The Integration of Fossil and Modern Forms

Such a crushed synopsis inevitably does an injustice to Grant’s thought, but it does show his broad sympathy with the aims of the leading anti-Cuvierians in Paris. In other ways, too, he emulated French thought: most importantly, his need to prove absolute continuity led to the integration of fossil and living forms. Often, only fossils could provide “the links which once connected the existing races” (42). He announced in his inaugural lecture (1828) that the “study of

the Animal Kingdom is inseparably connected with the science of Geology" (43), and that he planned to include the history of fossil species in his classes. Even though he was delivering a separate "Fossil Zoology" course by 1832, this historical understanding still informed his anatomical outlook, and he returned time and again in the comparative anatomy lectures to the theme of fossil succession and the limited duration of species. Of course, there was a reciprocal relationship: detailed studies of the physiology of existing species (of, e.g., gasteropods) were essential to elucidate the nature of extinct species known only by their shells, and therefore to throw light on "the past condition and revolutions of our globe" (44). Because of this was he able to use fossils to draw palaeo-environmental conclusions, for instance deducing from ammonite distribution the formerly tropical temperature of northern seas (45). More will be said later on Grant's fossil zoology, since it was the main context for his speculations on the generation of species.

4) The Reducibility of Biological Phenomena to Physico-Chemical Explanation

by the successful applications of the principle of chemical and mechanical science to the explanation of [an animal's] complicated functions, [we learn] that, notwithstanding the disturbing forces of the animal economy, which have hitherto defied all attempts at generalization, the true solution of all vital phenomena and the laws of organized beings are to be looked for in those magnificent arrangements which embrace the whole system of the visible universe.

Grant in his inaugural lecture in 1828
(46).

The most striking feature of his lectures, though, is that the whole “march of development” is conceived naturalistically. He sought no further for the explanation of life’s gradual development than the unity of plan and laws of animal organization, themselves special cases of mechano-chemical laws. He repeated in class what he had said in his introductory lecture, namely that organic laws “form as much a part of the great system of nature as the movements of the celestial bodies, and that the whole constitutes one grand and harmonious system of the material world” (47). His anti-vitalism was as pronounced as Geoffroy’s. He warned his students not to assume from the complexity of organic forms that they were governed by laws different from those in inorganic nature. There were, he admitted, a plethora of “counteracting agents influencing the complex forms of organization”, which made it in practical terms “almost impossible to trace in them the laws of inorganic nature”. But failure to discover such physico-chemical laws did not mean that they were inoperative. This is

not a sufficient reason for us to conclude, as many do in the present day, that the laws by which complex substances are governed, are something altogether different from the laws which regulate inorganic nature. Everywhere the natural philosopher and the chemist are making encroachments on the province of the physiologist. Everywhere do we find the laws of natural philosophy in operation in our bodies. Various physical instruments exist in animals; – acoustic instruments, pulleys, levers, hydraulic instruments, with all their valves, moving powers, fluids, &c.; but some philosophers are inclined to retard investigation, by assuming as a fundamental fact that the laws which govern

organic and inorganic bodies are totally different, and even at variance with each other (48).

This philosophic stance had certain theological corollaries. He never mooted Divine beneficence in the construction of animal forms. Occasionally he talked anthropomorphically of “Nature [having] ... an interesting and difficult problem to solve” (49). But he steered clear of Paleyite teleology (as did Knox); nor was there a hint of Sedgwick’s personally attentive Creator, or of a God somehow immanent. Grant’s was a hard Calvinistic deism. It was rare for him to allude to “the great Author of nature” at all, and if he did mention His “infinite wisdom, power, and goodness” (50), this was not as conceived by Anglican divines like Sedgwick or Buckland. The whole tenor of Grant’s thought suggests that it is better construed along deistic lines. In truth, Grant’s problem was not theology at all, but to produce a self-consistent materialist theory of life. Any wisdom and design were the result of the operation of uniform and immutable laws – and it was the “correct perception” of the “harmony” of these laws, he told the British and Foreign Institute in 1844, that enabled us to lay the foundation of “morality and virtue” (51).

Was Grant Teaching Development by Natural Generation in London?

[Lamarck’s] general views are distinguished by an extent and minuteness of observation, and originality and boldness of conception, which have thrown a new light over this intricate part of nature, although the novelty and singularity of some of his

results have retarded their general adoption (52).

To appreciate the reaction to Grant, we must establish the extent to which he broached development by “metamorphosis” in these lectures. In Edinburgh he assumed the descent of sponges, privately praised Lamarck, and published his approbation anonymously in Jameson’s journal. It was invariably fossil gradation and the Lamarckian series which prompted his theoretical speculations, so his most explicit statements might have been expected in the “Fossil Zoology” course. Unfortunately, no early manuscript of this exists; we do have his Swiney lectures on “Palaeozoology”, and these indeed show him unequivocally adopting “direct generation” (53), but their date – 1853 – is too late to let us conclude as much for the course in 1833. It is true that his thought, even his wording, varied spectacularly little over these two decades. Expressions in the 1853 MS crop up in the 1839 *General View*, as well as in the 1833 comparative anatomy lectures, increasing the *a priori* probability that he was broaching “direct generation” or the birth of higher from lower forms in the 1830s. (On the other hand, the social *context* of science in the tolerant fifties was dramatically different from what it had been in the thirties, and an amelioration of attitude towards transmutation would have made its public promulgation somewhat easier.) Actually, the *Lancet* lectures, even though restricted to comparative anatomy, do provide a good deal of relevant information; and it is more historically appropriate if we restrict ourselves

to this contemporary course. Since it was the 1830s and 1840s that witnessed the strongest reaction to transmutation, we need to know how far Grant was prepared to go at this time.

The first question is: Did Grant express himself unambiguously? And if his statements about “development” and metamorphosis were equivocal, how did his contemporaries interpret them? He maintained that one object of zoology was to inquire into “the origin and duration of entire species, and the causes which operate towards their increase or their gradual extinction; the laws which regulate their distribution, and the changes they undergo by the influence of climate, domestication, and other external circumstances” (54). His listeners, of course, could have interpreted this in totally conventional ways. Again, in his “Introductory Address” at the opening of the Medical School (1833), he reemphasized the gradualness of nature and resorted to metaphors which require extreme caution in handling. The comparative anatomist, he said, traces

the human organs coming successively into being, and rising in complexity from the monad through all the grades of animal existence, and discovers, by the close resemblance which exists between the transient forms presented by man’s organs during their development, and their permanent or adult forms in inferior orders of animals, that the plan of organization is everywhere the same, and man is the climax of its development. Extending his view to the remnants of organic beings embalmed in the earth, he finds that this kingdom has itself been gradually developed from simple to compound, that its roots are lost in the depths of the earth, and its extreme branches only are visible on the surface (55).

We should beware of using post-Haeckelian hindsight to give this metaphor an ‘evolutionary’ meaning, or at any rate a conventional Darwinian one: “roots” and “branches” need not imply common descent (that is, a forking ‘tree’) (56), nor do they carry any necessary transmutational implications. The whole could, for example, be conceived in Okenian terms, where contiguous species are related only in an ideal sense, being successive emanations from the Godhead. And Grant’s work does show evidence of transcendental influence, for example, in his transcendental osteology of the skull and vertebrae. On the other hand, we should not be too conservative in evaluating this root and branch metaphor. Remember that as early as 1827 he had implicitly considered a split between “parent” *Spongilla* and its marine offspring (the parent remaining unaltered in its freshwater habitat). And Fleming in 1829 had *interpreted* Grant’s results as pointing to a common albuminous *Spongia* “stem” splitting into respective silicious and calcareous sponge “branches”. At the very least, such implicit and explicit ‘forking’ images should caution us against automatically assuming that the 1833 statement was purely figurative.

Actually the semantic uncertainty of the lectures and very ambiguity of the situation could, as Martin Rudwick has suggested, have provided Grant with a convenient cloak (57). Take for instance his summing up in the *General View*, where he wrote that “the zoological productions, like the physical

features of our globe, have been subjected to constant and progressive changes from the period of the oldest Cambrian and Silurian rocks

The unity of the plan of organization, and the regular succession of animal forms, point out a beginning of this great kingdom on the surface of our globe, although the earliest stages of its development may now be effaced; and the continuity of the series through all geological epochs; and the gradual transitions which connect the species of one formation with those of the next in succession, distinctly indicate that they form the parts of one creation, and not the heterogeneous remnants of successive kingdoms begun and destroyed ... (58).

Again, the “one creation” image was decidedly Geoffroyan, and reiterated time and again by Knox, who put it in an avowedly transcendental context (59). So many of Grant’s statements were vague enough to have either transcendental or Lyellian counter-meanings (i.e. piecemeal replacement of discrete species). Such is true in a case like the following:

Animals succeed each other, generation after generation, like shadows on the earth, and entire species have their limited duration, which is but an instant, compared with the antiquity of the globe. Whole genera and tribes of animals, the roots of the existing races, have long since begun and finished their career, while time and life and development continue to roll on, and this great kingdom of nature, like the individuals that compose it, has had its embryo and its infant state (60).

At one point he did address the problem directly, only to brush it aside. “It matters not whether we fancy that those [species] now existing are the same forms changed by circumstance, or that there have been at every successive

instant new creations; naturalists differ on that speculation, – almost an idle speculation, where we can only arrive at a probability” (61). We know from a variety of sources that Grant *did* believe that these were the same forms “changed by circumstances” (62). However the words he used, like “development” and “metamorphosis” were ambiguous because of loose contemporary usage. Originally, “development” referred to ontogenetic growth, but by the 1830s and 1840s it was also used to denote fossil ascent – sometimes to mean a continuous progression of discrete species (63), at others a “generational” process involving either transmutation (perhaps catalysed by environmental change), or else a “higher generative law”, in Chambers’ sense.

One way to cut the Gordian knot would be to look at the attribution of meaning by contemporaries, who ought to have known what he was really implying. This carries risks (surely the best way to discredit a rival in the 1830s, after calling him an atheist, was to tar him as a transmutationist). Nonetheless there was a consensus among disparate parties (e.g. Darwin and Owen, who obviously viewed Grant from diametric positions) that some of his expressions did imply transmutation. Take for example Grant’s 55th lecture, where he admitted that species,

like the individuals which compose them, have also their limits of duration. The life of animals exhibits a continued series of changes, which occupy so short a period, that we can generally trace their entire order of succession, and perceive the whole chain of their metamorphoses. But the metamorphoses

of species proceed so slowly with regard to us, that we can neither perceive their origin, their maturity, nor their decay, and we ascribe to them a kind of perpetuity on the earth. A slight inspection of the organic relicts deposited in the crust of the globe, shows that the forms of species, and the whole zoology of our planet, have been constantly changing, and that the organic kingdoms, like the surface they inhabit, have been gradually developed from a simpler state to their present condition (64).

In 1841 Owen quoted this as “the latest terms in which the transmutation-theory has been promulgated, as supported by palaeontology” (65). Darwin also acknowledged it in the “Historical Sketch” prefixed to the third edition of the *Origin*. Even Grant himself reproduced it at the opening of his *Tabular View* (1861) to prove that he had never wavered from a transformist line. (Although his own testimony carries less weight; nothing is easier than to reinterpret one’s earlier statements, especially if they can be shown in a more favourable light.) But what makes it compelling is that Geoffroy, in publicly praising Lamarck’s two laws in 1825, had also used the word “metamorphosis” conformably with what we now understand as transformism, talking of “la transmutation et la métamorphose des parties” (66). Having already transliterated Geoffroy’s osteological and morphological terminology for his lectures (see below), Grant could not have been blind to the meaning Geoffroy invested in *métamorphose*.

So the evidence is fairly conclusive. Whatever the apparent ambiguity contemporaries understood Grant to mean

transmutation. Personally I am not convinced that he was being deliberately obscure. When he talked of “the natural chain of developments, running through the extinct saurian reptiles to the cetaceous mammals, to which they had many singular affinities” and then said of cetaceans that we should “seek for their origin” among the sauriens (67), I think he envisaged whales originating in “metamorphosed” reptilian stock. Nor should this appear very surprising, as Geoffroy was himself concerned with the mutability of fossil and recent reptiles at this time (68). Grant was never one to pander to public taste, and had he felt the need to disguise his views, he would surely have done better to avoid the subject altogether.

He certainly never tried to disguise his belief in spontaneous generation, even though this was as abhorrent as transmutation to elite geologists and naturalists (and as materialistic); not only Oxbridge divines like Sedgwick could thrust phrenology, spontaneous generation, and transformism into the same “sinkhole of human folly” (69). A more diluted opposition was met with on the ground; never as vehement, it was nonetheless detectable. Weissenborn admitted in 1838 that a man could still “incur the imputation of *atheism*, for presuming to observe the spontaneous generation of the *Acarus horridus*” (70). The *Magazine of Natural History*, by running Weissenborn’s German rationalist article – in which a *de novo* inorganic generation during each epoch was substituted for “a *Deus ex machina*, or a sort of Prometheus, who manufactured

the different animals and plants severally by mechanical means” – whipped up a storm, with replies from Edward Blythe and J. B. Bladon (71). Even ignoring the overt theological opposition to abiogenesis from those who were horrified by the prospect of contrivance without a contriver (72) – there remained medical scepticism, even on the hoary problem of the equivocal generation of entozoa (73) (less pronounced in the liberal press, which still countenanced *in situ* generation of gut parasites (74)).

Hodge has shown that for Lamarck and Chambers, at least, a *continuous* production of new life was necessary to prevent a ‘vacuum’ from appearing below as the infusoria began their upward migration (75). Whether Grant reasoned in this fashion is a matter of conjecture, since nowhere in print (that I can find) does he integrate his beliefs on spontaneous generation and fossil ascent. He considered that the simplest cells had formed spontaneously on the primitive earth; hence he was reported as saying (rather ungrammatically) in 1844:

Originating from atomic nuclei, almost verging on the mineral kingdom, and sole inhabitants of the heated waters of our primeval globe, the development of agastric animal cells, and of anenterous and enterodelous infusoria, has prepared the way for the higher tribes of animals...(76).

He also seems to have accepted a continuous contemporary production of infusoria and gut parasites. “From numerous experiments,” he said in his inaugural lecture, “Naturalists have been led to believe that the simplest organized bodies,

as *Monads* and *Globulinae*, originate spontaneously from matter in a fluid state, and that these simple bodies, of spontaneous origin, are the same with the gelatinous globules which compose the soft parts of Animals and Plants" (77). His reductionism was clearly conducive to speculations on the inorganic origin of life, and contemporaries were doubtless aware that his transmutation and equivocal generation were supported at this deeper level by a repugnant mechanistic philosophy.

How much did Grant influence the move away from Teleological Explanation in the early Victorian period?

Of two out of three English works, which are at present in the hands of the student of comparative anatomy and physiology, we know that the authors are largely indebted to him [Grant) for their materials

The *British and Foreign Medical Review* in 1842 (78).

Grant's lectures were more than a clearing house for imported Geoffroyan ideas in Britain. They were a detailed and practical working-out of Serres' recapitulationism and Blainville's series within the confines of a thoroughgoing transcendental morphology. Geoffroy himself, visiting London in 1836, embraced Grant as a "master" of philosophical anatomy and proclaimed him "le premier entre tous les savans" (79). The mercantile ideology of the Benthamite founders of LU had found expression in the progressive science of morphological reform, antipathetic both to static creationism

and its socially-unprogressive promoters: put bluntly, it could serve as a reformist tool to weaken Tory authority by undermining the power of the Established Church. Obviously materialist-transformist lectures emanating from the Godless college might be expected to have had a political appeal; and the fact that they *were* published and praised by radicals would have confirmed conservatives' worst fears. Thus, while the respectable *Medical Gazette* admitted on reviewing Grant's *Outlines of Comparative Anatomy* (the reworked lectures sold cheaply in parts) that he was "perhaps the most competent person in England to write a manual on the subject" (80), it nonetheless basted him unmercifully for leading students down the radical path (next chapter). Further left the press increasingly commented on the philosophical *framework* of the lectures (a subject conservatives understandably sidestepped, confining themselves largely to the factual content). Liberal journals, like the *British and Foreign Medical Review* (f. 1836 by John Forbes and John Conolly) and analytic *Medico-Chirurgical Review* (f. 1820 by James Johnson) greeted a lawful morphological approach enthusiastically. The *Medico-Chirurgical* had already treated Combe kindly, and proprietorial policy favoured a distinctly lawful line. The *Review* was even to admit in 1845 that the *Vestiges*' "doctrines have come out a century before their time" (81), damnable though such an endorsement would have sounded to Oxbridge geologists like Buckland, Sedgwick, and Whewell. The *Review* was the voice of progressive mercantile liberals in London, for whom naturalism more than materialism was the

dominant ideology. And for these middle-class progressives – as for the champions of ‘labour’ – transcendental morphology was a tool for attaining class ends, or rather for subverting the obstructionist scientific strategy of the aristocratic party, expressed through their Oxbridge Anglican mouthpiece. The *Medico-Chirurgical* constantly bemoaned the poor press philosophical anatomy received in England; its tenets having “too often met with the ridicule of those who have occupied our professorial chairs, a circumstance which can only be attributed to the generally-prevailing ignorance of the facts upon which the science rests” (82). Grant for them was the man “to dissipate this ignorance, deeply imbued” as he was “with the doctrines of the philosophic school”. It accordingly greeted his *Outlines* as infinitely superior to the dry factual compilations of Monro, Fyfe, and Blumenbach; claiming that the book “constitutes an era in the history of anatomy and physiology in this country”. It was guaranteed to rid English physiology of its crude disfiguring theories long since abandoned on the Continent. The *Review* supported his Geoffroyan theory of vertebral elements and articulate-vertebrate homology with only slight cavils. It went on, in a sympathetic review of Geoffroy in 1837, to raise Grant to the head of the handful of competent comparative anatomists at home (only Owen, Knox, and Grant’s protégé George Newport did it otherwise think worth mentioning) (83).

Adding:

[Grant’s] course of lectures, which adorned the pages of the *Lancet* two or three years ago, has contributed most essentially to the diffusion of

high and philosophical principles in the study of physiology; and we trust that Mr Wakley, with his accustomed zeal and enterprize, may be induced ere long to publish Dr Grant's lectures on this delightful branch of medical study [physiology]. There is certainly not a man in this country better fitted to do justice to the subject. His intimate acquaintance with human as well as with comparative anatomy, the variety and accuracy of his scientific attainments, the philosophical cast of his mind, and, withal, the impressive eloquence of his language admirably qualify him as a teacher of physiology (84).

It was, however, among the politicized radicals that Grant was *ecstatically* received. Wakley declared, as the last lecture appeared in his *Lancet*, that, despite all he had heard and read of Grant, he had not hitherto “formed the remotest conception of the strength and grasp of his understanding, or of the depth and extent of his researches”. The state of the subject before the professor “commenced his labours in the *University of London*” had made England a laughing stock; the shoe was now on the other foot, and “translations of his lectures into the French and German languages are already demanded in the foreign schools of medicine” (85). (*Outlines* was translated into German (86).) Never one to miss a trick, Wakley carefully noted Grant’s commendation in class of the *Lancet*’s crusade for “collegiate and corporate medical reform”, then disclaimed that “we cannot, from a false sense of delicacy, allow that circumstance ... to restrain us from expressing our unbiassed opinion of the great value of his scientific labours, and of the integrity, capacity, and vigour of his mind” (87). From then on the puffing continued unabated. Wakley was partly

blowing his own trumpet in 1836 when he claimed that Grant's "brilliant course" had attained "universal diffusion in the profession" through its publication in the *Lancet*, where it "constituted almost the only comprehensive and accessible source of information on this subject in the English language" (88). I will leave Wakley at this point, to take up his case more fully in the next chapter, when I shall examine the 'institutionalized' response to Grant in London. But the point has been made, that Grant's lawful, self-sufficient morphology had by far its greatest appeal for the medical Left, the reformist wing of the profession. Yet the political question is not completely disposed of, for I now want to examine Grant's influence – and the direction of comparative anatomy generally – in the 1830s, to suggest that, just as Parliamentary and municipal reforms (1832, 1835) gave greater democratic control to the middle classes, and political power shifted in a bourgeois direction, so the scientific power of the upper classes was concomitantly curbed in precisely the way urged by bourgeois organs like the *Medico-Chirurgical Review*. That is, we will investigate Grant's role *vis-a-vis* the crucial move away from Cuvierian teleology – the mainstay of the wealthy Oxbridge-Bridgewater elite – to a lawfully-constrained, morphologically-based science, in which the tinkering Paleyite Creator was replaced by a more culturally-appropriate Divine *legislator*.

Science historians have begun documenting the rise of a structural alternative to Cuvier's functionalism in Britain

in the 1830s, but have failed to recognize the contextual relevance of this, in relation to political events. Peter Bowler, in a survey of the better known publications, suggested in 1977 that we might usefully re-evaluate pre-Darwinian design to take stock of the growth of theological arguments based on unity of plan at the expense of Paley's watchmaker teleology (89). Ospovat has made a more satisfying internal analysis of the changing shape of the teleological argument. He concludes that 'non'-teleologists – those who rejected as all-inclusive the doctrine of final causes – still accepted perfect adaptation but failed to lock their organisms so specifically into single environments. The fact that an organism might fit many habitats left such scientists free to map a fixed plan onto nature, rather than being bound like Sedgwickians to perpetually modifying life in response to palaeo-environmental exigencies (90). Actually I think Ospovat's separation of the two camps is too extreme; time and again Owen *used* teleological adaptive explanations to score specific anti-Lamarckian points off rivals like Grant (91), even though in non-polemical situations he considered final causes as barren as Vestal Virgins and reasserted his primary allegiance to the morphological groundplan (92). Later in life, too, he consistently appealed to Cuvierian principles and final causes when deadlocked in Darwinian debates (93). Taking an 'instrumental' view and admitting that Owen's use of resources was sometimes guided by expedience rather than conviction tends to upset Ospovat's

hard and fast lines. But this is a minor cavil. On the major point I fully agree with Ospovat – in the 1830s we witness a shift towards theological design based on a *plan* in nature. *Or rather*, we observe a factional split, with conservatives clinging to older Paleyite notions, and younger London liberals following the modern trend (to varying degrees). Perhaps most interesting is that of the four non-teleologists Ospovat cites – P. M. Roget, Martin Barry, W. B. Carpenter, and Richard Owen – two had actually attended Grant’s lectures (Roget in 1832-3, Carpenter in 1834-5 (94)); while Owen had spent July and August 1831 studying in Paris with Grant, and was fully conversant with Geoffroyan thought. (Barry was actually trained by Tiedemann in Germany. His is therefore a case of direct debt to German transcendental thought.) Taking Carpenter, Roget, and Owen individually, however, reveals considerable ideological diversity in their uptake and application of the new morphology, demonstrating once again that while such bracketing might be heuristically valuable it belies the underlying subtleties that give social history its interest.

William B. Carpenter (1813-1885) later “looked back with peculiar interest” on Grant’s course, “not only for the information which he gained through it, but for the mental quickening and special love of the subject which it aroused within him” (95). Understandably so, because this Unitarian physiologist was very much a man of the *British and Foreign and Medico-Chirurgical* mould (he was actually to edit the

amalgamated *British and Foreign Medico-Chirurgical Review* from 1847 to 1852): son of a reformer and Catholic emancipationist, London/Edinburgh educated, and a defender of progressive lawful creation (it was Carpenter who patched up Chambers's heretical *Vestiges*). Like the *Review* ideal, he was passionately concerned with the underlying principles of philosophic biology, from a liberal-reformist perspective. In 1849 he took the Chair of Medical Jurisprudence at University College; shortly after he befriended the young T. H. Huxley, and became a merciless critic of Owen, thus demonstrating that Owen's *idealised* archetype had no monopoly on morphological thought (96). Often financially poor like his teacher (97), Carpenter alternated with Grant for the few paying positions: succeeding him as Fullerian Professor (1844), and preceding him as Swineyan Lecturer (1848). The Unitarian progressive was quick to appreciate the value of the Geoffroyan morphology offered him in Gower Street. By 1838 he was already using his favourite organ – the *British and Foreign* – to urge “general laws” against Cambridge conservative William Whewell’s final causes. Whewell’s “indignation” at Geoffroy’s speculations roused Carpenter to a spirited defence of the “higher” morphological laws which regulate animal structure. He considered laws the means by which the *prescient* Deity had adapted “the organized as well as inorganic creation” (98), arguing that they offered a more exalted conception of Omnipotence and revealed the true “vastness of that designing Mind, which, in originally ordaining them, could produce such

harmony and adaptation

The powerful hold of the new morphology over Carpenter was apparent during his stay in Edinburgh (1835-9), when he forged new and expansive “generalizations”. Discussing the “Unity of Function in Organized Beings” before the Royal Medical Society in 1837, he used the principle of connections to justify the transcendentalists’ analogy of wings and gills in aerial and aquatic insects. In the 1830s Carpenter was not focussing solely on the unity *within* Cuvierian divisions; at this time he too accepted that each natural group “passes by almost imperceptible gradations into every adjoining one”, and took as his prime example the “very gradual transition in the structure of most of the systems” between cephalopods and fishes. Despite Cuvier’s warning that “an impassable gulf” separates these divisions, “more extended researchers” tended to prove that

the nervous system, and internal skeleton of the highest cephalopodes, may almost be placed on a level with those of the lowest cartilaginous fishes; the arrangement of their circulating apparatus is strikingly intermediate between that of the mollusca in general and that of fishes; and whilst, in their organs of locomotion, we see a beautiful adumbration of those which are characteristic of fishes, so, in many fishes, we may trace the remains of those usually regarded as peculiar to the cephalopodes. No inferior group of mollusca presents such remarkable approximations to the class of fishes in any stage of development; and in none of them do we observe that symmetrical form and elongation of the trunk which is so prominent a feature in the structure of the naked cephalopodes (99).

He endorsed Lamarck’s invertebrate series, in which molluscs

and articulates rise from a common radiate base and progress in parallel towards the vertebrate level. And he implied that the vertebrate nervous system was an elaborate development of molluscous and articulate neural components. True, he was not prepared to accept “that the whole animal kingdom is formed upon the same type, and progressively developed in such a manner that the transitory states of the higher animals furnish exact representations of the permanent forms of the lower”, being in the process of assimilating Karl Ernst von Baer’s rival non-recapitulatory embryology of divergence. But working through the full implications of von Baerian embryology took a number of years, during which time a science of structural archetypes only slowly superseded his more Grantian model of continuity, where the *embranchements* stood connected. In his prize thesis at Edinburgh on the sensory and motor ganglia of invertebrates (1839), Carpenter criticized and extended Grant’s work and that of his student George Newport*, and supported the application of Marshall Hall’s reflex theory to non-vertebrates. In cephalopods he

* Because Grant incorporated his original discoveries in his lectures rather than publishing them, and because his protégés often reworked similar ground, priority disputes were inevitable. Newport’s case is striking for its personal component. George Newport (1803-1854) had been an apprenticed wheelwright before becoming interested in insects and studying under W. H. Weekes. He entered LU in January 1832. Because of his “distress” Marshall Hall recommended that all tuition fees be waived. Hall himself acted the beneficent patron, financing, feeding, clothing, and housing Newport (100). At Hall’s request, Grant inducted Newport free into his two classes, also touched by his “adverse circumstances” and “humble occupation”, and persuaded other professors to do likewise. In the 1832 session Grant first announced the motor

continued to see the nervous centres “protected by cartilaginous supports which obviously foreshadow the *neuro-skeleton* of *Vertebrata*” (104), and he sought precise analogies of the cephalopod’s ganglionic masses in the vertebrate brain, relating *Sepia*’s cephalic mass to the cerebral hemispheres, sub-oesophageal mass to the spinal cord, and so forth.

So a liberal progressive like Carpenter in the 1830s can be found working in what we might call a ‘Grantian’ paradigm. Even in *Principles of General and Comparative Physiology* (2nd ed. 1841), where he began emphasizing the structural integrity of each *embranchement*, he still believed that one division would show an “approximative tendency” towards another (105) – the nervous and osteological systems of the cuttlefish remained the “rudiment” of vertebrate organization. And even as von Baerian embryology began making an impact (it may have encouraged him to concentrate on the archetypes of each division, as it did Owen), his ideological

function of the abdominal nerve columns in articulates (his priority was substantiated by Hall (101)). When Newport in a series of Royal Society papers on the articulate nervous system seemingly plagiarized Grant’s work and then, adding insult to injury, took the prestigious Royal Medal in 1836 for his trouble, a bitter dispute broke out, the more acrimonious for Hall’s and Grant’s deep feelings of personal betrayal. Hall raised the indelicate issue of Newport’s uncouth behaviour and agitated on Grant’s behalf (not that Grant himself was incapable of giving the “ungrateful parasite” his due) and began proceedings at the RS to reconsider Newport’s award (102). The affair actually ran deeper than this, since the Secretary of the RS, P. M. Roget, had himself recently suffered taunts of plagiarism from Grant sympathisers, and was accused (unfairly) of partiality in the award, granting it as a kickback to Newport for help in polishing up his own ‘pirated’ Bridgewater (103).

use of the unity principle remained unchanged. He seems to have envisaged the structural archetype as a schematic or geometrical abstraction, not, as in Owen's case, as a Platonic ideal. More crucially, he continued to oppose traditional explanations using the new morphological theory of design. In the *Principles* he again complained of the extreme difficulty of proving "a Designing Creator ... from individual cases of adaptation of means to ends" (i.e. he rejected any harnessing of Cuvierian functionalism for Paleyite ends). He insisted that on witnessing the conformity of animals "to one comprehensive plan, and tra[c]ing] this throughout the extinct as well as the living beings of each type, no mind, capable of appreciating the value of cumulative evidence, can resist the inference, that such a plan *could* have originated no where but in Infinite Wisdom, and could have been executed by none but Infinite Power" (106).

The most revealing case of direct debt is Roget's. Peter Mark Roget (1779-1869) was the son of a Genevan Protestant pastor in London. He took his MD at Edinburgh in 1798, studied and taught at many of the London medical schools, knew Bentham, and acted as private tutor to a number of wealthy patrons (including the Marquis of Lansdowne, future President of the ZS, and Viscount Howick, who as Earl Grey was to steer the Reform Bill into law). Roget's failure to recognize seminal papers and irregular distribution of medals while Secretary of the RS (1827-48) made him many enemies, particularly among reformers: Grant, Hall, Wakley, and Robert

Lee were particularly incensed at his mismanagement of Society affairs (107). But Grant had other grievances. By 1830 Roget had been delivering popular lectures on animal physiology at the Manchester Literary and Philosophical Society, Russell and London Institutions, etc., for over twenty years. He was by then at the height of his fame, writing extensively for the SDUK, including their *Treatise on Animal Physiology* (1830) (108), becoming a Fellow of the Royal College of Physicians in 1831 and Fullerian Professor in 1833 (being Grant's predecessor at the RI). He was pious enough (and perfectly placed at the RS) to be selected to write one of the expensively-endowed Bridgewater Treatises at this time. (A series historians have often considered the apotheosis of the Paleyite tradition: indeed the contributions of Bell, Buckland, Whewell, etc., all praised Paley at the expense of the new morphology.) There is some evidence that Roget's knowledge might not have been up to scratch. At least, approached by Dionysus Lardner to write two volumes on "Animal Physiology" for the Cabinet Cyclopaedia, Roget remarked in March 1829 that he had "a tolerable foundation for the work in my written Lectures, yet I find that it will require considerable labour to bring up the subject to the present improved state of the science" (109). (After wrangling over copyright control the project fell through.) Anyway, in 1832-3 Roget paid £3 each to sit Grant's courses in Gower Street, boning up on the latest Continental approaches (110). There is no denying that his

Bridgewater *Animal and Vegetable Physiology*, proofs of which began appearing in summer 1833 not long after he finished the course, was suspiciously Grantian in tone and content. Roget now asserted that organic variation was not “indiscriminately followed” but “circumscribed with certain limits, and controlled by another law ... that of *conformity to a definite type*”. All existing forms were therefore “as so many separate copies” of a “certain ideal model” (111). Nor was this an endorsement of Cuvier’s *embranchements* because he continued:

To regard any of the beings in the creation as isolated from the rest, would be to take a very narrow and false view of their condition; for all are connected by mutual relations. Even among the leading types which represent the great divisions of the animal kingdom we may trace several points of resemblance, which show them to be parts of one general plan ... (112).

More gentle, perhaps, and showing greater deference to the superimposed Paleyite adaptations, Roget nonetheless propounded the Grantian Continental model, subordinating Cuvier’s functionalism to the higher morphology. He too positioned species intermediate between “adjacent types”, and proposed that “the steps of gradation by which one type passes into another, are so numerous and so regular, as to preclude the possibility of drawing a decided line of demarcation between those that properly appertain to each”. Like the philosophic anatomists, he imagined the “law of *Gradation*” to be a consequence of the unity of composition, since the latter left all “the races of animated beings ... members of one family” (113). And copying morphologists in

the wake of the Cuvier-Geoffroy debate, he took the cephalopod-fish bridge as his paradigm, finding that “From the Cephalopoda, the transition is easy to the lowest order of vertebrated animals” (114). He mentioned *Loligo*’s cartilaginous laminae as analogous to the spinal column (115); and he effectively endorsed Geoffroy’s and Grant’s theory of vertebral elements (116) and (more cautiously) discussed the vertebral skull, all in such a way as must have given fellow Bridgewater contributors like Bell palpitations. Of course Roget did not totally exclude convention, and he did adopt a reverential tone. On some points he was naturally loath to follow. Despite his lukewarm acceptance of successive creation, according to which “the standard types have arisen the one from the other” (a hypothesis, he admitted, which did explain the otherwise anomalous rudimentary organs), he refused to take the “transcendental” option and see this progressive development as the result of “simple laws”, let alone condone Lamarck’s “presumptuous reveries (117). But this could not disguise the fact that this book was a reverential reworking of transcendental morphology.

Grant was furious. Even while proof-sheets were circulating, he began agitating against this “improper use” of his lectures (118). Roget’s illustrations of vertebral elements and cranial vertebrae he considered nothing more than copies of his blackboard drawings. Roget excused himself on the grounds that scientific facts became the “property of those to whom they are communicated” and that he had every

right to make “whatever use” he pleased of knowledge imparted by “a public professor” (119). Not the best defence by any means, and he accordingly became known as “the plagiarist of Grant” in radical circles (120). But the nit-picking nature of the debate is not important; and we only concentrate on minutiae at the risk of missing the real issue. These piracy claims prove the impossibility for a “public professor” who fails to publish from protecting his property – particularly when he was in the process of selling it on the open market! Knowing Grant’s financial state, it must have rankled that Roget was making a fortune on the deal (probably about £1000 down, plus royalties on five editions up to 1869). And I doubt that Grant relished the irony of seeing his naturalistic lectures thrust into a theological context and sanctioned by the Bridgewater estate. One consequence of the affair for Grant was that – like Hall – he virtually disqualified himself from serving on the Council of the RS while Roget remained Secretary (or on the Physiology Committee which continued in its “invidious functions” oblivious to Hall’s pioneering reflex work (121)). This exclusion of reformers prompted Wakley in *The Lancet* to open up old sores a decade later (when Lee was assailing Roget for mishandling the medals). Wakley, with the politically bruising language for which he was famous, again accused the Secretary of jobbing and supplying medals to settle old debts. He demanded Committee reform and Roget’s removal (“nineteen years of place, and salary, and favouritism [are]

enough”, he exclaimed in 1846 (122)). Roget retired amid continuing controversy in 1848.

Grant’s failure (and in 1849 refusal) to sit on the RS Committee, as with his ejection from the ZS, highlights the extreme complexity of the ‘political’ situation. It was rarely a straightforward case of his intransigent Lamarckism and the community’s adverse reaction. A materialist philosophy was one reflection of his radicalism, which nevertheless manifested in innumerable other social, scientific and ultimately personal ways. On institutional reform his moralistic stand, unlike Wakley’s, was practically self-defeating, since his hatred of monopolistic practices at the RS, ZS, and College of Physicians, left him constitutionally incapable of kowtowing (unlike a less conscientious declinist like Babbage, who acknowledged the need for self-aggrandisement, and exploited the system while pitying its worth (123)). But while Grant was subject to a self-imposed exclusion order, the unreformed RS could, through Roget, absorb and sanitize his radical science, which inevitably tended to increase the bitterness. Although assimilation meant neutralization (enhanced by Roget’s flat rejection of transcendental laws and transformism), it is not until we look at the London allies of those like Whewell (considered “conservative reformers” by Cambridge standards (124)) that we finally see the doctrines appropriated, manipulated, and used against the very radicals who had first imported them.

While a Unitarian progressive like Carpenter could exploit the new morphology to undermine Established Church Paleyism, the reaction of a socially-aspiring Anglican anatomist like Owen to von Baer was wholly more complex. The older generation of eminent surgeons, typified by Sir Charles Bell, distanced themselves from the socially-deplorable consequences of Geoffroyism by simply disputing its foundation: the principle of connections. Bell in his Bridgewater denied that the theory of invariant elemental parts could possibly justify equating reptilian jaw joints and mammalian ear bones – rather, he argued with Cuvierian conventionality “parts are formed or withdrawn, with a never failing relation to the function which is to be performed” (125). By exposing the “pretensions” of morphologists he hoped to discredit Lamarckian and transcendental theories, which threatened to substitute “the cold and inanimate influence of the mere ‘elements,’ in a manner to extinguish all feeling of dependence in our minds, and all emotions of gratitude [to the “intelligent, designing, and benevolent Being”]” (126). But it was difficult for younger generation anatomists to swallow such undiluted Paleyism. Owen had travelled to Paris in 1831 in Grant’s company; there to be made aware of the strength of support for Geoffroy. At home his morphological studies were conducted in the Coleridgean atmosphere of the College of Surgeons, which fostered a conservative idealism (these points will be amplified in the

next chapter). He acknowledged that as the thirties progressed transcendental theories (e.g. of vertebral elements) steadily gained ground (127). Thus Owen was automatically immersed in the higher anatomy in a way that Bell never was. The problem and Owen's solution, as I shall present it in the next three chapters, might be encapsulated as follows. Owen was personally familiar with his metropolitan rival Grant, and kept a healthy respect for his learning. He frequently cited Grant's lectures and *Outlines*; he acknowledged his discoveries, for example, the nerves of *Beroë* (128), and in his own research on the homologues of the vertebrate skeleton was forced to deal with Grant's transliteration of Geoffroy's nomenclature, and his defence of vertebral skull theory, the opercular-auditory analogy, and so on (129). He knew that in London no less than Paris a unity between *embranchements*, with its auxiliary hypotheses of recapitulation and serial progression, supported a materialist interpretation of progressive development. His solution was to use von Baer's new embryology to split this conjunction of hypotheses, limit 'unity' to discrete divisions (each with its immutable archetype) and destroy recapitulation *in toto*, thereby retaining individuality during ontogenetic development. The resulting morphological science, though based on archetypes and fundamentally non-teleological, could be placed at the service of the institutional elite, the Oxbridge BAAS managers and GS oligarchs, as an ultra-modern, design-orientated rival to the synthetic materialist theories forged in Paris. This type of

interpretation becomes compelling when we set the actors in their correct political context. Once we understand Grant as a committed reformer in alliance with the doctrinaire radicals besieging Owen's College of Surgeons, then transformism becomes a direct political threat. The gentlemen surgeons had amassed immense power and wealth by monopolistic regulation of the profession; they now stood besiege by democratic 'Chartist' elements, as the Lords had been by the rabble, and they saw their interests lie in defusing this politically dangerous situation. Owen's 'brief', as Peel's brand of conservatism gained ascendancy in the country, was to forge a morphological science that would undermine the arguments for fierce reform. Thus it was not to be a reactionary Tory science, but a Peelite morphology of *cautious* reform, law-bound, but giving little comfort to those who would catastrophically redistribute power in the land. This is the subject I shall turn to now.

Notes and References

1. R. Owen, MS Notebook 5 (1831), BM(NH) L. Owen Coll. o.0.25.
2. R. E. Grant to T. Coates, 25 June 1833, UCL SDUK 30.
3. *The Lancet*, 1 (1835-6), 586. See also the opening editorial in R. B. Todd (ed.), *The Cyclopaedia of Anatomy and Physiology* (London, Sherwood, Gilbert and Piper, 1836), i, preface.
4. R. E. Grant, "Animal Kingdom", *ibid.*, i, 108-17. "The Cyclopaedia of Anatomy and Physiology", *British and Foreign Medical Review*, 1 (1836), 536-41. "We have been much disappointed in finding that his [Grant's] services have been so little called into requisition in the composition of one of the most meritorious works of the present day – we allude to the Cyclopaedia of Anatomy. Dr. Todd will do well not only for the credit, but also for the permanent success of his book, to entrust many of the physiological articles to such a man as Dr. Grant": "On Philosophical Anatomy", *Medico-Chirurgical Review*, 27 (1837), 83-128 (85).
5. S. L. Parker, "Lectures on Comparative Anatomy as Illustrative of General and Human Physiology", *Medical Gazette*, 7 (1830-1) passim. Parker later became Professor of Comparative Anatomy at Queen's College, Birmingham.
6. D. Ospovat, "Perfect Adaptation and Teleological Explanation: Approaches to the History of Life in the Mid-Nineteenth Century", *Studies in the History of Biology*, 2 (1978), 33-56; Ospovat, *The Development of Darwin's Theory: Natural History, Natural Theology and Natural Selection, 1838-1859* (Cambridge University Press, 1981); P. J. Bowler, "Darwinism and the Argument from Design: Suggestions for a Reevaluation", *J. Hist. Biol.*, 10 (1977), 29-43.
7. [R. E. Grant], "Baron Cuvier", *Foreign Review and Continental Miscellany*, 5 (1830), 342-80 (343).
8. T. A. Appel, "The Geoffroy-Cuvier Debate and the Structure of Nineteenth Century French Zoology", Princeton University Ph.D., 1975.
9. R. E. Grant, "Lectures", *The Lancet*, 1 (1833-4), 89.
10. G. Cuvier, "Nature", *Dictionnaire des Sciences*

Naturelles, 34 (1825), 261-8.

11. Appel, op. cit. (8), 358.
12. R. E. Grant, “Lectures”, *The Lancet*, 1 (1833-4), 95.
13. Ibid., 96.
14. Ibid., 124, 809.
15. Ibid., 121 , also 89.
16. “Dr. Grant on the Glandular System in the various classes of Animals”, *Medical Gazette*, 19 (1836-7), 749-50.
17. R. E. Grant, “Lectures”, *The Lancet*, 2 (1833-4), 1.
18. Ibid., 1 (1833-4), 767.
19. Ibid., 572-4, 539, 624, 703, 735, 767. “Development of the Vertebral Column”, *Medical Gazette*, 14 (1833-4), 425-6. Idem., *Outlines of Comparative Anatomy* (London, Bailliere, 1835-41), 57. Idem., *General View of the Character and the Distribution of Extinct Animals* (London, Bailliere, 1839), 40.
20. R. E. Grant, “Lectures”, *The Lancet*, 1 (1833-4), 573-4.
21. Ibid., 703; cf. R. Owen, “Hunterian Lectures 1 and 2”, *Manuscript Notes, and Synopses of Lectures. Owen: 1828-41*, BM(NH) OC 38, ff. 66-7; and idem., *Lectures on the Comparative Anatomy and Physiology of Vertebrate Animals. Pt. I Fishes* (London, Longman, 1846), 147-9 for his attack on Grant’s “exaggerated expressions”. I have commented on this in my “Designing the Dinosaur”, *Isis*, 70 (1979), 224-234 (232-3).
22. R. E. Grant, “Lectures”, *The Lancet*, 1 (1833-4), 770.
23. [Grant], op. cit. (7), 368.
24. R. E. Grant, “Lectures”, *The Lancet*, 1 (1833-4), 155-9.
25. Ibid., 351.
26. Ibid., 276.
27. Ibid., 195; cf. Grant, *General View* (op. cit.9), 60; “Palaeozoology Lectures”, BL Add. MS. 31,197, ff. 256-7.

28. R. E. Grant, Lectures, *The Lancet*, 1 (1833-4), 433.
29. Ibid., 96.
30. T. A. Appel, “Henri de Blainville and the Animal Series: A Nineteenth-Century Chain of Being”, *J. Hist. Biol.*, 13 (1980), 291-319 (304-5).
31. R. E. Grant, “Lectures”, *The Lancet*, 1 (1833-4), 816.
32. Ibid., 619.
33. Ibid., 2 (1833-4), 177.
34. Ibid., 1 (1833-4), 234.
35. Ibid., 270.
36. R. E. Grant, “On the Zoological Characters of the Genus *Loligopsis*, Lam., and Account of a New Species from the Indian Ocean”, *Proc. Zool. Soc.*, 1 (1833), 26-7.
37. R. E. Grant, “On the Anatomy of the *Loligopsis guttata*, Grant, and *Sepiola vulgaris*, Leach”, ibid., 90-1. Grant, “On the Anatomy of the *Sepiola vulgaris*, Leach...”, *TZS*, 1 (1835), 77-86 (79, 83). This might have been best received by the Quinarians at the ZS, as Macleay considered cephalopods an inosculant group connecting molluscs and fishes.
38. R. E. Grant, “Lectures”, *The Lancet*, 1 (1833-4), 505, 508, 512, 537-8; idem., *Outlines* (op. cit.19), 56.
39. Grant, “Lectures”, ibid., 505.
40. Ibid., 513.
41. Ibid., 2 (1833-4), 520.
42. R. E. Grant, *An Essay on the Study of the Animal Kingdom ...* (London, Taylor, 2nd ed. 1829), 11.
43. Ibid., 16-8.
44. R. E. Grant, “Lectures”, *The Lancet*, 1 (1833-4), 480.
45. Ibid., 510-1.
46. Grant, op. cit. (42), 5.
47. R. E. Grant, “Lectures”, *The Lancet*, 1 (1833-4), 127.
48. Ibid., 198, also 275.

49. Ibid., 135.

50. Grant, op. cit. (42), 7.

51. R. E. Grant, “On the Structure and History of the Polygastric Animalcules”, *Transactions of the British and Foreign Institute*, 1 (1844), 353-8 (353).

52. R. E. Grant, “Lectures”, *The Lancet*, 1 (1833-4), 95.

53. Grant, “Palaeozoology Lectures”, BL Add MS 31,197, ff. 17, 18, 20, 125, 251.

54. Grant, op. cit. (42), 6.

55. R. E. Grant, *On the Study of Medicine: Being an Introductory Address delivered at the Opening of the Medical School of the University of London. October 1st, 1833* (London, Taylor, 1833), 10.

56. We should note, in this context, that Grant’s mature picture was composed of not one, but numerous ‘trees’, presumably as a result of the continuous emergence of globular life: R. E. Grant, *Tabular View of the Primary Divisions of the Animal Kingdom* (London, Walton and Maberly, 1861), 9.

57. Pers. comm.

58. Grant, *General View*, op. cit. (9), 60.

59. R. Knox, *The Races of Men* (London, Renshaw, 1850), 443.

60. R. E. Grant, “Lectures”, *The Lancet*, 1 (1833-4), 953.

61. Ibid., 2.67.

62. The evidence comprises 1) the articles in Jameson’s Journal, 2) Darwin’s testimony, 3) attribution of meaning by Owen (note 65 below), 4) the “Palaeozoology” MS; and 5) the *Tabular View*.

63. Bowler distinguishes between continuous and discontinuous progression in *Fossils and Progress* (New York, Science History Publications, 1976).

64. R. E. Grant, “Lectures”, *The Lancet*, 2 (1833-4), 1001.

65. R. Owen, “Report on British Fossil Reptiles, Part II”, *Report of the British Association for the Advancement of Science* (Plymouth, 1841), 197. C.

Darwin, *On the Origin of Species* (London, Murray, 3rd ed. 1861), xiv. Grant (op. cit. 56), iii.

66. Geoffroy St.-Hilaire, “Recherches sur l’Organisation des Gavials”, *Mémoires du Muséum d’Histoire Naturelle*, 12 (1825), 97-155 (151).
67. R. E. Grant, “Lectures”, *The Lancet*, 1 (1833-4), 701.
68. Geoffroy St.-Hilaire, “Divers Mémoires sur de Grands Sauriens …”, *Mémoires de l’Académie Royales des Sciences de l’Institut de France*, 12 (1833), 1-138.
69. J. W. Clark and T. M. Hughes, *The Life and Letters of the Reverend Adam Sedgwick* (Cambridge University Press, 1890), ii, 83.
70. W. Weissenborn, “On Spontaneous Generation”, *Magazine of Natural History*, 2 (1838), 369-81 (370).
71. E. Blythe, ibid., 507-9, anticipated Huxley’s line (used in his case against creationists) that it was difficult “to imagine an extraordinary coincidence of circumstances concurring to produce one adult elephant” (508). See also J. B. Bladon, ibid., 4 (1840), 280-6, 339-41.
72. J. L. Drummond, “Thoughts on the Equivocal Generation of Entozoa”, *Annals and Magazine of Natural History*, 6 (1841), 101-8. P. Keith, “Of the Conditions of Life”, *Phil. Mag.*, 10 (1831), 32-40. Bladon, op. cit. (71).
73. “Mr Rhind on Worms”, *London Medical and Physical Journal*, 61 (1829), 142-53. “Mr. Bushman on Animals found in the Blood”, *Medical Quarterly Review*, 1 (1834), 364-6.
74. “Dr. Scouler on Worms”, *Medico-Chirurgical Review*, 13 (1830), 221-3.
75. M. J. S. Hodge, “The Universal Gestation of Nature: Chambers’ *Vestiges* and *Explanations*”, *J. Hist. Biol.*, 5 (1972), 127-51 (140-6); idem., “Lamarck’s science of Living Bodies”, *Brit. J. Hist. Sci.*, 5 (1971), 323-52 (329, 341, 344, 345).
76. Grant, op. cit. (51), 358; also Grant, op. cit. (56), 3.
77. Grant, op. cit. (42), 18.
78. “Dr. GRANT’S *Outlines of Comparative Anatomy*”, *British and Foreign Medical Review*, 13 (1842), 217-9 (218).
79. Geoffroy St.-Hilaire to R. E. Grant, 10 September 1836, published in BS, 691-2.

80. *Medical Gazette*, 15 (1834-5), 508-9.

81. *Medico-Chirurgical Review*, 1 (1845), 147.

82. “Outlines of Comparative Anatomy”, *ibid.*, 23 (1835), 376-88 (376, 378, 380).

83. “On Philosophical Anatomy”, *ibid.*, 27 (1837), 83-128 (87, 106).

84. *Ibid.*, 85. Actually Johnson personally hated Wakley, and there was constant sparring between the *Lancet* and *Medico-Chirurgical*; but this should not be used to obscure the deeper political orientations of the journals, or their *de facto* agreement on the need for reform. On personal distrusts, see C. Brook, *Battling Surgeon* (Glasgow, The Strickland Press, 1945), 48-9.

85. *The Lancet*, 1 (1834-5), 689.

86. R. E. Grant, *Umrisse der Vergleichenden Anatomie* (Leipzig, Otto Wigand, 1842).

87. *The Lancet*, 1 (1834-5), 689.

88. *Ibid.*, 1 (1835-6), 586.

89. Bowler, op. cit. (6).

90. Ospovat, “Perfect Adaptation”, op. cit. (6).

91. Especially in his work on the “Teleology of the Skeleton of Fishes” in Owen, *Lectures on the Comparative Anatomy and Physiology of Vertebrate Animals*, op. cit. (21).

92. R. Owen, *On the Nature of Limbs* (London, van Voorst, 1849), 9-10.

93. A. Desmond, *Archetypes and Ancestors: Palaeontology in Victorian London 1850-1875* (London, Blond & Briggs, 1982), 172-4 for further discussion.

94. “Professors’ Fees Books” MS, UCL; *University of London: Medical Classes*, Records Office UCL.

95. J. E. Carpenter’s Introductory Memoir in W. B. Carpenter, *Nature and Man: Essays Scientific and Philosophical* (London, Kegan Paul, Trench, 1888), 10.

96. Desmond, op. cit. (93), 16-7, 37-8, 41, 92-3, 212-3 (n.39). Among the few recent works on Carpenter are D. Ospovat, “The Influence of Karl Ernst von Baer’s Embryology, 1828-1859”, *J. Hist. Biol.*, 9 (1976), 1-28;

and V. M. D. Hall, “The Contribution of the Physiologist, William Benjamin Carpenter (1813-1885), to the Development of the Principle of the Correlation of Forces and the Conservation of Energy”, *Medical History*, 23 (1979), 129-55.

97. W. B. Carpenter to P. B. Ayres, 12 June 1851, Wellcome Institute AL; this concerned Grant’s testimonial which Ayres was getting up – Carpenter was forced to decline any committee work, “my, time being very fully occupied in those labours, by which alone a poor Physiologist, with a family to maintain, can make a living in this country”; and he could only subscribe two guineas, “which is as much as ... I feel justified in appropriating out of a very limited income”.

98. [W. B. Carpenter], “Whewell’s History of the Inductive Sciences”, *British and Foreign Medical Review*, 5 (1838), 317-42 (338-9).

99. W. B. Carpenter, “On Unity of Function in Organized Beings”, *ENPJ*, 23 (1837), 92-114 (97-8).

100. “Letter from Dr. Marshall Hall”, *The Lancet*, 1 (1837-8), 748-9; “Mr. Newport’s Reply to Prof. Grant and Dr. Marshall Hall”, *ibid.*, 812-7 (816); R. E. Grant, “Further Observations on Dr. Hall’s Statement regarding the Motor Nerves of Articulata”, *ibid.*, 897-900.

101. “Dr. Marshall Hall on the Nervous System”, *ibid.*, 650.

102. R. E. Grant, “Reply to Mr. Newport’s Insinuations respecting the writings of Dr. Marshall Hall and Dr. Grant”, *ibid.*, 746-8 (747). “Parallel Passages by Marshall Hall, M.D.”, *ibid.*, 2 (1837-8), 17-8 (17).

103. *The Lancet*, 1 (1846), 391, 499-501, 634-6.

104. W. B. Carpenter, *Prize Thesis. Inaugural Dissertation on the Physiological Inferences to be deduced from the Structure of the Nervous System in the Invertebrated Classes of Animals* (Edinburgh, Carfrae, 1839), 44-5.

105. W. B. Carpenter, *Principles of General and Comparative Physiology* (London, Churchill, 2nd ed. 1841), 191.

106. *Ibid.*, 192; cf. 560.

107. *The Lancet*, 1 (1846), 351. R. M. MacLeod, “Of Medals and Men: A Reward System in Victorian Science 1826-1914”, *Notes and Records of the Royal Society*, 25-6 (1970-1), 81-105 (89-90).

108. [P. M. Roget], *A Treatise on Animal Physiology* (London, Baldwin & Cradock, 1830).

109. P. M. Roget to D. Lardner, 8 ALs 1828-9 Wellcome Institute (see letter dated 19 March 1829).

110. University of London: Medical Classes, Records Office UCL. R. E. Grant, “Dr. Roget’s Bridgewater Treatise”, *The Lancet*, 1 (1846), 445-6.

111. P. M. Roget, *Animal and Vegetable Physiology Considered with Reference to Natural Theology* (London, Pickering, 1834), i, 48, 49.

112. Ibid., 51-2.

113. Ibid., 407, 52; ii, 627-9.

114. Ibid., i, 268.

115. Ibid., 263 note.

116. Ibid., 388-96.

117. Ibid., ii, 637-8.

118. “Letter from Dr. Roget”, *The Lancet*, 1 (1846), 420; Grant’s reply, ibid., 445-6; and the rejoinders, ibid., 482-3. R. J. Godlee, “Thomas Wharton Jones”, *British Journal of Ophthalmology*, 93 (1921), 145-81.

119. *The Lancet*, 1 (1846), 483.

120. Ibid., 1 (1836-7), 624.

121. R. E. Grant to T. Bell, 5 March 1849, ibid., 1 (1850), 88.

122. Ibid., 1 (1846), 636.

123. M. Crosland, “Explicit Qualifications as a Criterion for Membership of the Royal Society: A Historical Review”, *Notes and Records of the Royal Society*, 37 (1983), 167-89 (179-83).

124. M. M. Garland, *Cambridge Before Darwin: The Ideal of a Liberal Education 1800-1860* (Cambridge University Press, 1980), vii.

125. C. Bell, *The Hand: Its Mechanism and Vital Endowments as Evincing Design* (London, Pickering, 1833), 139.

126. Ibid., 144.

127. Owen in 1839 wrote that Geoffroy’s “analysis of the vertebra in the abstract has been generally adopted in this country”: R. Owen, “Report on British Fossil Reptiles”, *Report of the British Association for the*

Advancement of Science (Birmingham, 1839), 43-126 (46).

128. R. Owen, “Hunterian Lecture IV”, *Manuscript Notes, and Synopses of Lectures. Owen 1828-1841*, BM(NH) OC 38, f. 39.
129. R. Owen, “Report on the Archetype and Homologies of the Vertebrate Skeleton”, *Report of the British Association for the Advancement of Science* (Southampton, 1846), 169-340 (253; also 232, 241, 259).

Chapter 5

The Response to Grantism in the 1830s: The Case of Richard Owen

If ... past science is viewed as an activity which was socially organized and countenanced, we may expect it to have shown sensitivity in various degrees to some of the diverse elements in its social environment. This claim should receive particular justification from the study of the scientific activity of individuals who were associated with institutions in which sciences were taught, especially during a time of persistent stress and repeated crisis.

J. B. Morrell in 1971 (1).

Morrell was considering the *Theophobia Gallica* of Scots Tories in the aftermath of the 1789 Revolution. But an *institutionalized* scientific response to contemporary political uncertainties can be demonstrated for other periods, and in none was it more pronounced than in the 1830s. Agitation for social change and parliamentary reform peaked with the revolutionary crises of 1831-2. Failure to realize Chartist demands resulted in the regular eruptions of mob violence in many cities throughout the decade – rampaging which caused considerable anxiety for liberal gentlemen like Lyell among the ruling elite. The need for public order cut across party lines; and both Whig and Tory, savants conspired to isolate the kind of disaffected materialist philosophy which could fan the flames of working-class discontent. Such was the threat from below that they were backed by many middle-class reformers: the SDUK set (e.g. Roget and Greenough) and Benthamite ‘legalists’, intent upon reducing both nature and

society to order – those whose demands had already been met in large part by the Reform Bill of 1832. So it is to the doctrinaire radicals that we must turn, as representing the dispossessed unenfranchised masses in the country; those agitating for *continuing* reform to satisfy more than solely middle-class needs. I will argue that the link between politically-based and class-bound radicalism, Lamarckian materialist philosophy, and the conservative response fostered in elite Coleridgean institutions like the Royal College of Surgeons (RCS) was direct and demonstrable, and that it provides a unique insight into the *socio-political* basis for the failure of Lamarckism to transfer successfully to the capital in the Reform years.

To achieve this I want to concentrate on Grant's main metropolitan rival, the morphologist and palaeontologist Richard Owen, and his 'Peelite' strategy for tackling the materialist threat while allowing a cautious conserving reform of comparative anatomy. Owen's case is important for raising a number of fundamental interpretive issues, in particular the way in which the social conditioning of a scientist was affected. Having set Lamarckism against this broader socio-political canvas, the way will be clear for a more subtle analysis of the wider cultural factors favouring the rejection of transformism and Geoffroyan morphology (transcending historical accounts which concentrate solely on their supposed internal inadequacy for Oxbridge divines and

their protégés, this was largely a post hoc justification). In short, the political associations of a transformist like Grant will help us better understand the vehemence of Owen's response, *qua* Anglican anatomist and Establishment socialite employed in a College effectively besieged by Chartist sympathisers.

Taking this approach allows the crossing of a number of methodological boundaries. Most important, we have the example set by social historians of science, particularly Morrell and Thackray in their pioneering study of the ideological use of science and financial resources by the gentlemen of the British Association (2). More explicitly political is MacLeod's recent study of institutional reform of the learned societies, a reform which paralleled parliamentary changes (3). Although we are dealing with social ideologies of elite scientists and their political context, I am very much concerned with the *content* of their science: with the cultural creation of new morphological and anatomical resources – and of the *scientific* validity of the product which makes the current historiographical techniques of sociologists of knowledge like Shapin (4) extremely useful. In chapter 6 I will investigate Owen's restructuring of the anatomy or morphology of monotremes, apes, and fossil saurians for ideological ends, and his ‘marketing’ of the final product, using what Barnes calls ideas as ‘tools’ to achieve his anti-Lamarckian ends. (And finally, by suggesting in opposition to an older generation of positivist-Whig

historians that science is what you make it, I am also endorsing the kind of liberationist ideology of science history advocated by radical historians like R. M. Young (5.).

Besides these sociological guidelines, we have available new procedural tools forged on the shop floor by recent recruits to the Lyell Industry. Retooling by younger scholars like Bartholomew, Ospovat, Bowler, and Corsi means that my study of Owen owes more to the Lyell than the Darwin Industry. In the latter sociological analyses and studies of motives are still unfashionable as interpretive devices, and the Industry itself remains inward looking. By shifting our focus onto Owen, we gain one advantage over Lyell scholars, who have had to back-project from the sexagenarian Lyell's *Species Journals* to his "religious" and psychological motivations in the 1830s. Owen can be placed in a strictly contemporary setting, since we have detailed manuscript material available for the 1830s. No hindsight need be involved and we can keep our actor firmly on a contemporary stage. Also we can gain a better understanding of Owen's social and political appreciation, and the way this might have affected his science, something that has not been attempted with any degree of consistency for Lyell. In other respects Owen makes an equally rewarding study. In Chapter 7 I will suggest that his anti-transmutational ideology, far from being a *retarding* influence (which might be claimed in Lyell's case, where it produced an unacceptable non-

progressionist palaeontology), on the contrary served a positive heuristic function. It was intimately related to his cautious reform of the science: to the development of his archetypal morphology and von Baerian palaeontology in the 1840s – aspects of his science which have been the focus of recent scholarship (6). Thus the present study is relevant to the more ‘internalist’ researches of Ospovat on Owen’s von Baerian morphology, and Bowler on his model of fossil divergence. By studying Owen’s attitude to Grant’s transformism on a number of interrelated levels, we can begin to understand *why* he pioneered these new approaches (which Bowler calls “revolutionary” (7)). And since his morphology was formulated in a Coleridgean context and was designed to sustain the anti-mercantilist ideology of elite surgeons we have a means of tackling the social basis of the new science.

The Multi-Dimensional Nature of the Threat: Owen, Grantism, and Reform

Following Bartholomew’s study of “Lyell and Evolution” in 1973, scholars have become increasingly interested in the response of gentlemen geologists (8) to Lamarckism during the reign of William IV (1830-7). However, workers in the Lyell Industry have often been indifferent to the outstanding social characteristics of Lyell’s London. While *Principles* (1830-3) was being written the country underwent political mobilisation, suffered often violent demands for reform, and entered a potentially revolutionary situation. Whether

revolution was really possible during the Reform crises of 1831-2 is a moot point.

Interpreters of the working class movement like E. P. Thompson believe that

“Viewed from one aspect, England was without doubt passing through a crisis in

these twelve months in which revolution was possible” (9). Others like James

Hamburger argue that revolutionary rhetoric was a radical strategy designed to

frighten the landed classes into making concessions, not to fashion the conditions

from which the overthrow the state could follow (10). The operative point, however,

is not whether revolution was possible, but how the landed classes *perceived* the

danger to their privileged position. This is the more important to know for savants

who were also gentlemen of substance: the managers and oligarchs in charge of

learned bodies who financed the production and distribution of knowledge. Any

study of Lyell which ignores this turbulent background and concentrates solely on,

say, his “religious” or aesthetic (11) objections to Lamarckism (important as these

are), is missing a potentially fruitful area of study. It is not true that he was

indifferent to the gathering crisis. Lyell’s first lecture at the Anglican King’s College

in the Strand (the fact that he *actively* chose the theologically orthodox King’s and

turned down the radical LU is itself telling (12)) coincided in May 1832 with the peak

of popular agitation. Yet Martin Rudwick, who has convincingly argued that Lyell

was more comfortable in Tory King’s surroundings, notes that his lectures contained

no hint of the inflammatory situation concluding that Lyell was a “non-political

careerist”, i.e.

his “totally unpolitical attitude was explicitly related to his belief in the political powerlessness of the nascent scientific profession” (13). But the fact that his lectures made no mention of the street battles does not preclude a more subtle moulding of their contents, such that they might support the social pretensions and respectable institutions of the landed class, or at least undercut those heretical geological philosophies which threatened to excite an already inflammatory situation. Rudwick himself has made an excellent case for Lyell’s social responsibility. But for a *Quarterly* reviewer writing for Tory anti-reformers this surely implied more than simply pious theological obligations. Lyell’s backer for King’s, Adam Sedgwick (a Whig in the older Fox mould), was not alone in seeing socially disruptive consequences in French godless materialism. Lyell played a central role in Morrell and Thackray’s elite “scientific clerisy”, whose function at the GS and BAAS (f. 1831) was *inter alia* to create the sort of cultural resource by means of selective patronage which might have a stabilizing social influence. Thus through institutions, scientific theories themselves could be turned to political advantage. This thesis is perhaps easier to substantiate in Owen’s case, where we can assess the *political* importance of his scientific response by its effect on radical anti-monopolists bent on democratizing his College of Surgeons. (I hope to achieve this without sacrificing a proper evaluation of his science, or minimizing his more often mooted moral or

theological objections to bestial transmutation; rather, I would depict these as inextricably related to his social objectives and part of the larger picture.)

First, however, I want to examine current Lyell scholarship to highlight its strengths and weaknesses. Having established these, we will be in a better position to tackle Owen from a broader, politically-contextualized perspective.

The Example Set by the Lyell Industry and same of Its problems

We shall never be able to unravel Lyell's thought processes, but it seems to me probable that the prospect of evolution affronted his beliefs about the way God had ordered the world, and that his faith in the providential pattern of earth history was so powerful that it regulated, or governed, his selection and adoption of 'scientific' theories. Consequently when, in 1827, he perceived that he could construct a coherent account of the history of life which squared with his providentialism, excluded the possibility of evolution, and explained the phenomena, that option was obviously the one he took up.

Michael Bartholomew in 1973 (14).

Despite their difference of geological opinion, Whigs like Lyell and Sedgwick (indeed most liberal Anglicans, including Tories like Buckland and Whewell), were sensitive to the same extremist elements in society – the reactionary Evangelical cosmogonists on the one hand, and democratic materialist pedagogues on the other. They gave short shrift to the ultraconservative Scripturalists; Sedgwick, exasperated in 1830 by continuing attempts to found Christian geology solely on

moral testimony, hurled shafts of invective against Andrew Ure’s “deformed progeny of heretical and fantastical conclusions” (15). It wasn’t simply that cosmogonists at home were professionally threatening, although they did deny the inductive foundation on which the GS managers rested their temporal power (trenching, as Morrell and Thackray say, on the scientific *authority* of the liberal Anglicans). With Grand Tours of the Italian states *de rigueur* to complete a gentleman’s education, fellows like Lyell, Murchison, and Buckland had actually encountered what they considered the ‘inquisitorial’ consequences of “Granville-Penn” thought. They witnessed repression first-hand in Papal states and knew from experience the social consequences of fanatical biblicism. In 1828-9 Lyell toured the Italian States. In Naples he wrote to Mantell that “the inquisitorial suppression of all cultivation of science, moral or physical, is enforced with unrelenting rigour, and considerable success” (16). (Eighteen people had been shot for political offences the day before he passed through Salerno.) Revealingly, it was from *Rome* that he wrote (on learning of Ure’s £500 advance from Longmans for a “Hebrew cosmogony” proving that “we ought all to be burnt at Smithfield”) his famous letter to his sister in which he claimed to have “got a rod for the fanatics” (17). Lyell’s livelihood of course hinged on the continuing hegemony of liberal Anglicanism – a fact brought home to him during his tour. In an unpublished letter written after his return, he told Mrs. Whitby that

I was not a little glad to reach Paris...to escape from countries where the Jesuits are now opposing in the most barefaced & systematic manner the free publication of opinions on matters of science as well as politics; when one has almost every where the misfortune of seeing every man who has distinguished himself either dispossessed of his professional chair or under surveillance & suspicion. They took away my Italian books on Geology on entering the frontiers of Piedmont saying that I should find them at Geneva for no scientific works could be opened in the Sardinian dominions. But for my travelling companion ... I should certainly have lost them altogether but they just reached me in time before I quitted Geneva. In France one breathes a healthier atmosphere again...(18).

So on the one side Anglicans were responding to authoritarian intolerance which they imagined to be the practical upshot of Jesuitical literalism. Buckland even voted against Catholic Emancipation in 1829 because of his Sicilian experiences (191). The repressive *political* implications of unrestrained Scripturalism were frighteningly apparent, and Lyell's intent to "free the science from Moses" was not a secularist's scientific attack on the church but an act of social *responsibility* (20)). It was a scientific stand *in support of* Anglican moderation.

Exactly the same could be said of Lyell's response to the threat from the Left; from on the one hand atheistical French Lamarckism, and on the other radical philosophies which threatened democratic extremism and mob rule. Indeed, the challenge from this side might have been viewed as a piece, since Lamarck's godless materialism threatened to destroy the threads of moral responsibility holding the masses in

restraint, and extreme radicals (some of whom were transformists) were tapping this potentially immense source of political power and urging the destruction of monopolistic institutions, landed power, and Anglican hegemony.

Can such a re-contextualized *political* interpretation of Lyell's science be sustained? Well, Bartholomew has argued categorically against it. According to him, Lyell differed

from men like Sedgwick and Wilberforce in that he does not seem to have regarded the preservation of man's unique status as a precondition for the maintenance of ethical and political standards. Unlike Sedgwick and Wilberforce, it did not occur to him that an evolutionary history for man might signal the collapse of Western civilization (21).

Yet Bartholomew himself has given the most persuasive account of the heuristic value of an anti-transmutatory ideology – arguing that Lyell's antipathy to Lamarckism was so strong that his non-progressionistic palaeontological response actually worked against the uniformitarian grain of *Principles*. The thrust of Bartholomew's article lay in the premise that Lyell's ulterior motivation rested in some deep-seated and undiscoverable “religious” objection based on the inviolability of human dignity: “Lyell's distaste for anthropoid origins is much more ‘psychological’, or perhaps aesthetic The sources seem to be more personal, and are probably inaccessible to us” (22). Cannon, Hooykaas, and Rudwick have pointed out that we need to remain inordinately cautious of Lyell's own self-justifying reconstructions, and Porter has highlighted the propagandist element in *Principles*

(23). So Bartholomew is right in trying to get behind Lyell's own 'internalist' justification, which centred on the cumbersomeness of Lamarckism, which he saw cluttered with enough *ad hoc* hypotheses (inexorable progression, spontaneous generation, etc.) to make it *scientifically* unacceptable (24). But despite detailing the chronology of Lyell's switch – his progressionism in 1826, reading of Lamarck in 1827, realisation that the theory would eventually have men the progeny of orangs (25), and subsequent shift to nonprogression – Bartholomew has few *contemporary* sources to prove that fear of bestialization and the threat to human nobility made the *Philosophie* anathema. Not that I doubt him. I believe that some fear of this sort was the proximate cause of Lyell's rejection of the doctrine. He was familiar with the anatomical similarity of man and ape (26) and knew that Lamarckian progressionism would result in an ape acquiring reason. Therefore in *Principles* he denied that a trained orang could rival an elephant in intelligence, or that it possessed such faculties "which can countenance the dreams of those who have fancied that it might have transmuted into 'the dominant race'" (27) (a response presumably to the materialist Bory St. Vincent – next chapter). Although one can cite statements from *Principles* illustrating Lyell's horror at the prospect of transmuted man renouncing "his belief in the high genealogy of his species" (28), Bartholomew himself was often forced (confessing his unease at the prospect) to resort to Lyell's *Scientific Journals*

written a quarter of a century later (1855-61). Here he finds the “religious” component he is looking for: the “concern, even mania, for the status and dignity of man” (29). Sure enough three decades later Lyell did appear obsessed with saving mankind’s dignity – making him the product, not of environmental chance, but superior moral ends. As he jotted in 1859:

if the truth were confessed & the apprehension lurking in the depth of the soul were drawn out, if the secret misgivings were divulged, it is...fear lest the dignity of Man in the relations to the Universe shd. be lowered by establishing a nearer link of union between him & the inferior animals then his conscious feeling of superiority, his hope and aspirations naturally lead him to indulge aspirations without which he becomes in his own eyes a creature of the moment, incapable of earnest thought & of sacrificing for the good of others, doomed to be as ephemeral & insignificant in himself (30).

But as others have pointed out, extrapolating back thirty years raises a fresh set of problems (31). The older Lyell possibly had different preoccupations; and social amelioration (the fifties, with the demise of Chartism, were peaceful, affluent times) might have left him more time to ponder man’s spiritual destiny where before his social fate was of concern. There is also some reason to believe that the constitutive theological element in Lyell’s thought only goes so far in elucidating his rejection of Lamarckism in the 1830s. His Christian conception of causation, understanding of natural law as a Divine mandate, and belief in beneficent design, did not themselves rule out a theory of providential species substitution. Moreover, his comments on Babbage’s proofs of his

Ninth Bridgewater suggest that this book did not come as “cold comfort”, as Cannon claimed (32); actually the unpublished correspondence suggests that he was quite happy with an over-arching law by which the Almighty programmed species adjustment (33). Of course he did not mean this necessarily to apply to an ascending fossil series (in which case man might be implicated), but to his steady-state system. Still it suggests that in the later 1830s Lyell could conceive of a model (based on Babbage’s computer program) which would simulate natural-supernatural species origination, and that he probably did not, as Bartholomew has argued, regard “evolution” as the “*only conceivable* naturalistic explanation of species origination” (34). Still, this says nothing about what Bartholomew contends was Lyell’s motive for shoring up palaeontology against the ravages of Lamarckism, his belief in a special human dignity – motives which Bartholomew comes close to ascribing to Lyell’s conscious state (35). Better before we ascribe mental states that we examine the sort of cultural conditioning that Lyell would have experienced. We should at least look at contemporary social, political, and religious factors which might provide clues to understanding his obsession with “dignity”. Bartholomew fails to make contact with social history; yet during the months of the Reform Bill crises when Lyell was penning his anti-Lamarckian volume England suffered greater social ferment than at any other time during the century. I am not suggesting that Lyell’s obsession with preserving “dignity” was some immediate, self-

serving mechanism to protect his own social position. However, as an advocate familiar with the working class call for reform, a *Quarterly* reviewer, and son of a high Tory who had just taken over the family estates at Kinnordy, his scientific activity did bear a peculiar parallel to the need to preserve the sanctity of polite society. (The one thing Lyell cherished, almost above political allegiance, was gentlemanliness – his appreciative recognition of this trait features time and again in his letters.) It is unlikely that he was uninfluenced by political events. Although a Whig advocate of university reform and model of ‘gentlemanly’ liberalism, his *Quarterly*-King’s affiliations suggest that he was not unhappy in Tory surroundings. “I must not sport radical,” he joked to Mantell, “as I am become a *Quarterly Reviewer*” (36), but it was no laughing matter when *Quarterly* Tories were vehement anti-reformers. He was preeminently a leisured gentleman; he might have worried over money matters and have taken to writing in order to stand on his own feet, but he remained partially supported by his landowning father. Like liberals everywhere he appalled at the fate of the “many millions of our labouring classes”, telling his father that “we must congratulate ourselves at not being among [them]”. And adding “I am quite clear, from all that I have yet seen of the world, that there is most real independence in that class of society who, possessing moderate means, are engaged in literary and scientific hobbies...” (37). As a liberal he naturally welcomed Wellington’s defeat and supported Grey’s ministry, but in common with most Whigs he had no truck

whatever with the more violent attempts of the labouring unions to destabilize the situation. Lyell himself had actually witnessed mob violence in Paris during the July Revolution. He wrote to his sister his famous sentence about the immense ages that would be required “for Ourang-Outangs to become men on Lamarckian principles” after witnessing mobs rampaging through the streets for two days outside his house (38). His sister kept him informed about the election at home in 1830 while he was geologizing on the Continent, and he worried over the “mob-rule which I see daily in the papers”, all of which made him “ashamed of our system” (39).

Lyell’s belief in “dignity” had more than metaphoric significance, and clearly ran beyond scientific confines to inform his political and social understanding. Indeed it partly explains his political ambivalence: although he welcomed Grey into office, he had great difficulty restraining himself from voting Tory from the personal qualities of the candidate. Leonard Wilson points out that while writing the anti-Lamarckian volume of *Principles* Lyell was greatly “distracted by the turbulent state of politics” (40). The evidence of Lyell’s *Life* and Wilson’s biography bears out the difficulty under which he wrote. He penned the chapters for Murray while residing at his father’s estate at Kinnordy. The Forfarshire election in September 1831 was contested for the Tories by Lord Airlie’s brother, Donald Ogilvy, who held the estate next to Mr. Lyell snr. and who

commanded his anti-reformist vote. Even Charles thought him “so gentlemanlike a man ... as compared to Halliburton [his Whig opponent] that I wish with all my heart I could vote for him” (41). At times the radical threat was personally felt: while he was writing in October 1831, his five sisters out in a carriage were “*hissed*” by “a mob of about 40 reformers” and at first feared being stoned (42). Lyell’s belief in gentlemanly liberal qualities, his ambivalent Tory sympathies, and the threat to his Tory family, would certainly have increased his distaste for man’s Lamarckian reduction to lowly parentage, when it was precisely the mobs of humble parentage who were menacing his family. Everything about Lyell bespeaks his need to maintain an image of decorum in face of social upheaval: his preference for the refined Anglicanism of King’s rather than *embrouillés* radicalism of Gower Street, his attempts to smuggle a little liberalism into the Tory *Quarterly*, the designful anti-Lamarckism of *Principles* supporting man’s “high genealogy”. This understanding of “dignity” that informed the wider aspect of his social, political, and religious thought could have entered *Principles* as a timely reminder to working-class pedagogues that man’s high estate was divinely instituted, and that the social order and natural place were not to be tampered with.

So contingent political factors, and Lyell’s reaction to them, suggest the possibility of a deeper social explanation of his rejection of Lamarck’s materialistic, socially-

levelling doctrines. Indeed in *Principles* we find tantalizing strands of corroborative evidence, where Lyell issues a cautionary note about the consequences of Lamarckism. An uninitiated naturalist presented with this “visionary doctrine” of directional and progressive development would throw all caution to the wind, Lyell wrote in an imaginary scenario:

Henceforth his speculations know no definite bounds; he gives the rein to conjecture, and fancies that the outward form, internal structure, instinctive faculties, nay, that reason itself, may have been gradually developed from some of the simplest states of existence, – that all animals, that man himself, and the irrational beings, may have had one common origin; that all may be parts of one continuous and progressive scheme of development from the most imperfect to the more complex; in fine, he renounces his belief in the high genealogy of his species, and looks forward, as if in compensation, to the future perfectibility of man in his physical, intellectual, and moral attributes (43).

How are we to read this? The unwary naturalist being lulled by poetic fancies into throwing over his high genealogy in favour of socially reformist tonics? Meliorism and promises of earthly improvement now substitute for divine redemption. Sinister social and religious consequences could follow if man’s station were compromised. This passage seems clearly aimed at the meliorist promises of radicals and reformers, promises of heaven on earth when rank and privilege were abolished: for Lyell this could be disqualification enough.

The power of Lyell’s anti-Lamarckism to affect associated aspects of his theory has been studied by Ospovat. He agrees that Lyell’s desire was “to preserve man’s special place in

creation” (44) but refuses to place Lyell’s geology in a separate category from his biology contending that these disciplines were strategically related in *Principles* and that Lyell’s commitment shaped his science as a whole. Ospovat argues that because Lyell was an environmental determinist his non-directional organic history *demanded* a theory of inorganic nature comprising “a nonprogressive series of climatic conditions” (45). Ospovat then shows how, unable to *refute* the evidence of directional temperature change in earth history (his European travels in 1828-9 had convinced him that the evidence was sound), Lyell set about countering its purported explanation in terms of declining central heat (46). His alternative was to make the post-Carboniferous temperature increase merely a small, directional segment of a greater geologic cycle – which was not due to any overall planetary cooling, but to a migration of continental masses away from equatorial latitudes in Carboniferous times to their present-day northerly sitings. Ospovat’s conclusion is that Lyell’s faith in man’s spiritual status and moral tie to the Creator influenced more than just palaeontology, extending deep into his understanding of environmental causes.

Despite this demonstration of the operation of powerful moral interests in the making of gentlemanly geology, neither Bartholomew nor Ospovat looks beyond Lyell’s reading of Lamarck and apparent idiosyncratic need for human “dignity” as a explanation. But surely a relevant question is which

section of society was sympathetic to Lamarck that Lyell could be so frightened? And what social and political objectives did his doctrine sustain? Questions like these are important because Lyell admitted that Lamarck had his worshippers and in *Principles* stated that transmutation “has met with some degree of favour from many naturalists, from their desire to dispense, as far as possible, with the repeated intervention of a First Cause” (47). In other words he is himself imputing wider motives to the Parisian deists. Or is his finger pointing at their British supporters in the secular wing of the reform movement?

Most scholars agree that Lyell feared the application of Lamarck’s doctrine to the progressionist edifice because it would leave man a transformed ape (48). Pietro Corsi argues that a conservative British response became imperative when Lyell “saw signs of the diffusion of transformism in England itself, where it could even form an unholy alliance with prevailing progressionist and directionalist interpretations of the history of life on earth” (49). Since Grant is the only British university teacher whom we are familiar with who had actually mated transformism to a progressionist fossil series and powered it with a cooling-earth motor, he must be considered a prime suspect for having alerted Lyell to the danger. Bowler and Bartholomew have both mooted a connection, if only by way of footnote, suggesting that Grant’s 1826 *ENPJ* paper praising Lamarck must have been known to Lyell (50), although neither presented any evidence beyond the timing of

events (51). The circumstantial evidence that Lyell knew of Grant and his work is nonetheless strong and might be stated as follows: Lyell was familiar with Jameson's journal (52), the main organ for Grant's papers between 1825-7; he was on intimate terms with Fleming, who gave encouragement to Grant and may also have known of his Lamarckian predilections. Lyell was also acquainted with Leonard Horner (whose daughter Mary he became engaged to in 1831 (53)), and Horner as geologist and Warden of the University was active in introducing Grant to GS members in the late 1820s (54). Lyell was conversant with the affairs of the University, including its financial troubles (55), and had himself firmly scotched rumours that he was a candidate for the geology chair in 1829 (56), leaving Lindley, Turner, and Grant to their tripartite geological arrangement. Indeed, right through the period when Lyell lectured in the Strand, Grant not a mile away was delivering his summer "Fossil Zoology" course which advocated progressive serial development and directional climatic change – and presumably (as in his comparative anatomy course) the metamorphoses of species. Lyell was certainly familiar with Grant's inability to make people pay to be taught, using his case to point out the difficulties (which Lyell himself was experiencing) of the laissez faire system. He gives the impression of having been aware of Grant's predicament all along. He noted in 1833 that

Grant lectures *gratis* at the Zoological Society to overflowing rooms, but the moment he began at the London University [i.e. in 1828], for a trifling

fee, only about eight or ten students came (57).

The cumulative evidence of so much circumstantial evidence is compelling. But the most intriguing piece of evidence concerns Grant's role in Lyell's own Geological Society. Grant was a visitor at Ordinary Meetings from 1828 and was elected a Fellow in May 1830, whereafter he regularly introduced students and visiting savants as guests. In 1831 he paid his composition fee and was elected onto the Council in 1832 (58). Thus in the year that Lyell published his anti-Lamarckian volume, a transformist had penetrated his sanctum sanctorum, the Council chamber of the GS. Here he must have rubbed shoulders with Lyell, although lacking documentary evidence we do not know Lyell's exact feelings on the matter.

Impressive as this evidence is, I do not want to belabour the point; it is important to follow Corsi and see Grant as one of a number of transformists and materialist deists, most admittedly Continental, whom Lyell knew or knew of – so many, in fact, that he was probably convinced that Lamarckism remained a potent force. His fears could have been confirmed in 1828-9 and 1830, during his two Continental trips, when he became acquainted with the *savans* who contributed to the (often materialistic) multi-volume *Dictionnaire Classique des Sciences Naturelle* (17 volumes published in 1822-31). He spent six weeks in September-October 1830 studying the conchological cabinet of Paul Deshayes (1797-1875) in Paris. (It was while working on this collection that he witnessed

the mobs in the streets below.) It was Deshayes' estimates of the enormous periods required for the appearance of new variants that prompted Lyell's statement about the immense ages necessary for the transmutation of ape into man. Deshayes had been Lamarck's associate, was preparing to republish *Animaux sans Vertèbres* (1833 ed.), and in the *Dictionnaire* praised his mentor's "spirit of analytical philosophy" (59). At this time Lyell also met Deshayes' collaborator André de Féruccac (1786-1836), anti-catastrophist and believer in gradual global cooling, originally a Charles X man but now, with the revolution, hoisting his own *tricolor* flag (60). Corsi believes that Lyell would also have read the materialist articles of the *Dictionnaire*'s editor Bory St. Vincent, and that he was familiar with Geoffroy's support for Lamarck (61). So Lyell was exposed in the revolutionary and anti-clerical Parisian climate to materialist philosophy, anti-catastrophist progressionism, and Lamarckian biology; and he could have been seriously perturbed on his return to find a Francophile materialist like Grant on his own GS Council.

The Use of Richard Owen as a Subject

I have dealt at length with current Lyell scholarship in order to underline historiographic trends and their social limitations before proceeding to Owen's case, where contextual social and political issues are more easily discernable; also Owen's response can be located in a precise institution-

al locus. Bartholomew is certainly wrong in stating that “Lyell was alone in scenting the danger of the result of adding transmutation theory to fossil progression” (62); indeed, in some ways Richard Owen provides a more instructive example of the scientific reaction to the threat of bestialization. Treating Owen as a subject has a number of advantages: we can provide a social explanation without recourse to inaccessible mental states, and we can keep close to the manuscript and printed material for the 1830s. As a result we can anchor the debate more firmly into the social context. In short, by concentrating on an aspiring Anglican in the employ of a Coleridgean monopolistic college which was the target for radical attack, we can treat this anti-Lamarckian response as an integral part of the wider reaction to the threat of reform and perhaps revolution in the 1830s.

So my reasons for singling out Owen are straightforward. We can identify the transformists with whom he was acquainted, and possess MS and other evidence of his changing views of them. Also the important differences between Owen and Lyell work to our advantage. Owen as a young anatomist was not wealthy. He had to earn a living, unlike Lyell who was supported by his father and handsomely remunerated by Murray for *Principles*. While Lyell could afford to resign his teaching post at King’s, Owen had to seek extra lecturing income besides his curatorship and (from 1836) Chair at the Royal College of Surgeons. This left him more

‘institutionalized’, so to speak – he was subject to certain institutional conventions and his response to Lamarckism or medical reform had to be compatible with official College thinking. Since the RCS was itself under attack from medical reformers in the 1820s and 1830s, some with radical and transformist connections, we have a main institutional framework for interpreting Owen’s response. It is also the case that Lamarckism was as relevant to London zoologists as geologists (or more so), and since the embryo Zoological Society played host to both Owen and Grant, we can plot the social consequences of Owen’s anti-Lamarckian gambits *inside* the Society, and document the decline of Lamarckism in terms of its shrinking power base.

Owen is an attractive subject for one final reason. Until the last decade, British sociologists and philosophers have largely understood *ideology* as functioning to generate biases or deviations from ‘true’ science. By such positivist historiographical canons, Lyell might be said to have been led away from rational inquiry to produce ‘bad’ science, bad even in the operational sense of unacceptable to his contemporaries (the Great Year entered into no geological calendars). Older historians argued that cognitive sociology was impotent to explain the generation of true theories, or rather that the claims made for sociology of knowledge were “not reflected in its concrete achievements” (63). More recently Bloor, Barnes, Mackenzie, Young, Shapin and others (64) have argued cogently for the role of social interests in

the shaping of scientific knowledge on the assumption that such knowledge holds no special status as ‘rational’ or transcendent. Barnes argues that

Increasingly, knowledge is being treated as essentially social, as a part of the culture which is transmitted from generation to generation, and as something which is actively developed and modified in response to practical contingencies ... Its generation cannot be understood in terms of psychology, but must be accounted for by reference to the social and cultural context in which it arises. Its maintenance is not just a matter of how it relates to reality, but also of how it relates to the objectives and interests a society possesses by virtue of its historical development (65).

Here and in the next two chapters I hope to apply the Barnes-Shapin ‘instrumental’ model to elucidate Owen’s scientific strategy in the Reform years. Certainly his anti-transformist ideology was as powerful as Lyell’s; it led, for example, to his innovative approach towards monotremes, apes, and saurians (Ch. 6). But more importantly I shall argue that it had a strong heuristic value, leading to his development of good, socially-acceptable science. Lyell’s non-developmental palaeontology was a cultural failure; and Lyell’s defence of it became so strained that Owen himself in his Presidential Address to the GS in 1851 lamented that “the Philosopher should have suffered to subside so far into the Advocate” (66). Friends like Murchison, Sedgwick, Conybeare, Whewell, Owen, and even Scrope, were at a loss, as Bartholomew says, “to account for his [Lyell’s] decision to reject one of the secure generalizations [progressive ascent of fossil life] that palaeontologists had drawn from the preceding thirty

years' work" (67). In the 1830s geologists might have disagreed on the extent and cause of discontinuities in the fossil record, but few disagreed that there was an overall progressive trend (almost all elite members of the GS were "discontinuous progressionists" in Bowler's sense (68)). One has only to read Lawrence's papers on the collective assessment of Lyell's theory and Elie de Beaumont's directionalist rival with its causal dynamics based on diminishing igneous forces to appreciate how effectively Lyell was bypassed (69).

Owen's ideology, on the other hand, was crucial to the development of his archetypal morphology and von Baerian palaeontology in the 1840s (Ch. 7). From these arose his formulation of progressive divergence, which Ospovat considers the most sophisticated theory of its day (70). Owen's conceptions, unlike Lyell's, were also considered the highest "generalizations" in their own day (71). Therefore Owen's anti-transformist ideology had a *positive* heuristic value. It led to a piece of 'good' science. This should be emphasized because of the overriding impression given in many Whiggish studies that early Victorian anti-evolutionary strategies were a *retarding* factor; studies that themselves uncritically accept the bourgeois secularist historiographies of late nineteenth century professionalizing Darwinians like Huxley, Tyndall, and Lankester without inquiry into their partisan character (72). In fact, Owen's morphological science of progressive divergence was central to the biologi-

cal synthesis of the early 1850s; and since Darwin's branching conception could be mapped onto it, Owen's von Baerian palaeontology, cleansed of its idealistic archetypal imagery and turned to new ideological ends, could actually have been incorporated in the new evolutionary edifice (73).

How Did Owen Make Contact with Lamarckism as a Living Issue

If Owen saw Lamarckism as a 'threat' it could have been because it was presented in a form which imperilled some aspect of his political, religious, or social beliefs; or because its *adherents* posed some form of professional challenge – for jobs, society posts, etc. This would have been more than merely competition for available resources. The incursion of radical transformists into the Council chambers of learned bodies like the GS and ZS would have jeopardized Owen's ideal of the socio-political form such societies should take. A 'threat' also has the aura of immediacy about it – it implies that he encountered Lamarckism as a *living* issue. Lamarck might have died a blind pauper in 1829 and been ceremoniously interred by Cuvier in a disparaging *Éloge* (74), but I agree with Corsi that transformism was no straw man in the 1830s. In this section I suggest that Owen was made forcefully aware of its strengths in London and Paris at this time, and that he was personally familiar with its advocates, whom he, for a time at least, counted among his friends. Personal intimacy is important to

establish; no amount of dispassionate reading of Lamarck's books could have brought home to Owen the *viability* of transformism. He had to see for himself how it was being applied. And I shall suggest that his awareness of its appropriation by radical-reformers made his 'Peelite' scientific response the more politically urgent.

During the July Revolution Cuvier had opportunely slipped into England. At the RCS he met Owen, then a young Assistant Curator (and the only French-speaking member on hand who was familiar with the anatomical preparations (75)). Cuvier's reciprocal invitation resulted in Owen's making his first trip to the Jardin, the heart of the "Macedonian Empire" (76) of comparative anatomy, in July 1831. It would have been surprising if Owen's contact with the Parisian *savans* had not influenced him profoundly, even if, as his grandson speculates, he "seems to have regarded this stay in Paris as an exceedingly pleasant and entertaining holiday" (77). Little is actually known of his trip, which lasted from late July until early September. His grandson did publish one letter written from Paris to William Clift, in which Owen mentions his introduction to Cuvier and seeing Geoffroy, Blainville, Latreille, and others at the Institute. But mostly it relates his enjoyment of the Royalist fireworks at the arrival of Louis-Phillipe (78). Unfortunately nothing is said of his views concerning the recent riots or the rampant anti-clericism of the educated classes (and mobs) which had fuelled the July Revolution (79). Yet he could hardly have

missed the burnt-out shell of the Archbishop's palace next to Notre Dame, demolished during an anti-legitimist riot in February. A respectable aspiring Anglican, already a recipient (as his mother proudly notes (80)) of Cuvier's patronage, could not have been blind to the republican mood.

There is however one document not mentioned in the *Life* which is crucial for understanding Owen's experience in Paris, since it shows how he might have been introduced to the contentious issues in French comparative anatomy. Owen kept a pocket notebook during the trip in which he jotted social engagements and scientific observations. From this we learn that he stayed in the Hotel du Jardin du Roi, Rue Copen, and that Grant – summering in Paris as usual – was another guest. It shows Owen becoming increasingly friendly with the Scots Lamarckian. On innumerable occasions – five that Owen mentions – they breakfasted or dined together, sometimes seeking out new restaurants for the purpose (81). They accompanied one another to lectures, with Owen learning who was old-hat and who up to date (82); and they, engaged in deep (and not so deep) discussions. Grant regaled Owen with stories of his travels:

Wednesday 10th

To the Gardens, Dr Grant & I took breakfast together. Dined at Richards, returned & had coffee together; the Dr gave me an account of his wanderings in Germany, Austria, Bohemia, Italy, &c.

Sunday, [14 August] Breakfast with Dr Grant, met Mr Hart, Lecturer on Anat. at Dublin and Dr Alex. Thompson. Had long conversation de omnibus rebus

anatomico physiologico-mathematico-nonsensicology afterwards walked into the Gardens to see the Animals (83).

And so on. This evidence of their growing acquaintances and of Grant's seniority, experience, and willingness to show Owen the ropes, carries tantalizing possibilities. Remember that Grant was a personal friend of Geoffroy and had collaborated with him the previous year on the controversial question of the oviparity of *Ornithorhynchus* – a subject intimately connected with contemporary transformist taxonomy which Owen was to take up. Given Grant's familiarity with the Muséum and its professors, it is easy to imagine him taking the inexperienced Owen in hand. Grant of course had an irrepressible nature and an unrestrained enthusiasm for Lamarck (as the teenage Darwin had discovered at Edinburgh five years earlier). And considering that the celebrated Academy debates between Cuvier and Geoffroy had only taken place the previous summer, and that Geoffroy was currently working out the precise sequence of teleosaur transformations (84), it is likely that Grant burst forth in equally high admiration for Geoffroy and his palaeontological endeavours. Did canvassing for Geoffroy in Owen's presence induce the young anatomist to take an interest in the transcendental debate? He did jot in his notebook: "Wednesday 17th [August] Bought *Philos. Zoologique* read till 11 ..." (85). Whether this was Lamarck's *Philosophie Zoologique*, reissued in 1830 (probably because Geoffroy was taking up the question of species "metamorphosis"), or Geoffroy's own account of the

Academy clash with Cuvier, *Principes de Philosophie Zoologique* (1830), isn't clear. But it is tolerably certain that Owen was now intimately aware of the transformists' case. And having been introduced to it from a partisan perspective he was well placed to judge its strengths. (He also, incidentally, recorded four days later that he had visited Lamarck's *maison* "but could not get in" (86).)

Other entries confirm that Grant was present at Cuvier's *soirees*; and that, as we suspected, he was active in introducing Owen to the dignitaries. One of these entries also confirms that Owen heard at first hand of Cuvier's grievances against Geoffroy. Here we also see the problem of ape anatomy mentioned for the first time (Owen having just dissected a dead orang at the ZS):

Saturday [20 August] ... In the Evening at Cuvier's. Mad. Cuv. & Madlle Duvance to both gave Mr and Mrs Clifts regards on which they returned kind regards stayed till 11. Cuvier shook hands at going away – Had a long convers. about Orang with him. He said he had never dissected a Chimpanzee, was going to write upon [?Sternum] contra Geoffroy. Dr Grant introduced me to Fred. Cuvier, who enquired what had been done with our dead Giraffe (87).

Had Owen been induced to view French science through Grant's eyes, he would have seen the relevance of Lamarckism to unidirectional development and Geoffroy's unity of composition, which for Grant held throughout the series. He would have found the Lamarckian series supported to differing degrees by Grant's mentors Geoffroy and Blainville: Geoffroy vindicated Lamarck's transformist laws, while Blainville

adopted the series partly in political opposition to Cuvier (88). Grant and Geoffroy were materialist deists, envisaging life as an extended whole, as one creation, and opposed to Cuvier's discrete *embranchements*. Certainly within six years Owen was disentangling this knot of heretical concepts – transformism, unity extended across divisions, and the unilinear series (Ch. 7) – whose logical relations he was undoubtedly made aware of in Paris in 1831. But even more frightening for an Anglican Peelite during the Reform crisis must have been the realization that Geoffroy's materialist morphology was being appropriated in England by extremists. It was being pressed into anti-aristocratic service and used to undermine Anglican hegemony and the Oxbridge ideology of the Coleridgean managers of the chartered corporations.

Preconceptions: The Coleridgean Influence

The Paris episode provides evidence of Owen's exposure to Lamarckism as a *living* doctrine. Actually Grant and Owen might have met earlier; Owen had attended Barclay's Edinburgh classes on anatomy and surgery between October 1824 and April 1825 (89). (It was Barclay who advised Owen to continue his apprenticeship under John Abernethy at St. Bartholomew's.) Since the ailing Barclay delegated the invertebrate part of his course to Grant in 1825 they may have met then; Owen may also have seen the 'savage' transcendentalist Knox, who went into partnership with Barclay that year. The evidence that he

had read some of Lamarck's works before visiting Paris is more convincing. One of his early reading lists (on paper watermarked 1828 and therefore drawn up while working on the Hunterian catalogues in Lincoln's Inn) includes Lamarck's *Histoire Naturelle des Animaux sans Vertèbres*, and unspecified papers by Lamarck and Grant (90). Owen also took notes from one of Grant's articles on the ova of zoophytes and he dissected the *Octopus ventricosus* with Grant's ENPJ paper in front of him (91). This diligence in learning at least part of his invertebrate anatomy from Scottish sources increases the possibility that he had seen Grant's anonymous papers in Jameson's journal; nor is he likely to have missed continuing reports of Geoffroy's researches on monstrosities, carried out to establish the "uninterrupted succession of the animal kingdom ... effected by means of generation" (92).

Despite this, there was really no substitute for seeing, meeting, and listening to the philosophic deists in Paris to appreciate the strength of their position. All of which makes it important to analyse Owen's preconceptions, to understand his framework for interpreting French morphological science. The evidence is that in London he had already been encouraged to view Lamarckian materialism from an antagonistic romantic perspective (and the subsequent direction of Owen's thought tends to bear this out). He moved to the capital in 1825 to continue his apprenticeship under John Abernethy (1764-1831) at St. Bartholomew's Hospital. Abernethy, as President of the RCS, went on to help Owen become assistant to Clift in the

Hunterian Museum. Abernethy's own attack on William Lawrence's materialistic lectures had brought him to the notice of the poet and Germanizing philosopher S. T. Coleridge (1772-1834). Coleridge was of course fiery in his denunciation of the mechanisms and atheisms of the Anglo-Gallic corpuscular philosophy, onto which he blamed the industrial evils (93), and which had led, in his view, to the Lawrencian aberration. As he told to his confidante and literary executor, the surgeon Joseph Henry Green (1791-1863)

A system of Materialism, in which Organization stands first, whether composed by Nature or God, & Life &c as its *results*; (even as the sound is the result of a Bell) – such a system would, doubtless, remove great part of the terrors which the Soul makes out of itself; but then it removes the Soul too, or rather precludes it (94).

The RCS had a powerful Coleridgean lobby. Coleridge knew the College well; and from his interest in medical philosophy, residence with the surgeon James Hillman, and sympathy for gentlemen like Abernethy and Green, we might imagine that his scientific delegation to the National Church mooted in *On the Constitution of the Church and State* (1830) would have been composed of Lincoln's Inn men. Abernethy attended Coleridge's lectures at the Crown and Anchor in 1818-9 (95), when the teacher tried “to insinuate into Mr Abernethy” more philosophic means of repulsing “the attacks of Lawrence, and the Materialists” (96). Clearly it was this background of *medical* disputes which gave Coleridge's lectures their topical appeal. He wrote to William Mudford of the *Courier*

when considering the best means of advertizing them:

Of what transcendent general interest the subject is (Materialism in relation to Logic and Physiology as well as to morals & religion) you know & feel – and the hot controversy between Abernethy & Lawrence and the *impudent* Work by Sir C. Morgan, give it an additional temporary interest (97).

Coleridge attacked “dogmatical materialism”, took Abernethy’s side in denying the materialist doctrine that life proceeds from organization, and in his MS “Theory of Life” criticized Lawrence by name (98). Despite his dislike of Abernethy’s crude vitalism, he realized the political importance of lending his support, and this strengthened after Abernethy published his “eminent” *Physiological Lectures* in 1817 (99). Abernethy for his part quoted Coleridge against Lawrence, and in 1819 lashed out publicly at Lawrence’s “hostile and taunting expressions” (100). “By their fruits should trees be known” was Abernethy’s maxim, intelligible to Lincoln’s Inn listeners in this period of post-Napoleonic reflection. He insinuated that while godless materialism in France might have been used to throw off the yoke of “superstition and bigotry”, it had nonetheless engendered terror on the streets and the Napoleonic wars, and he apocalyptically quoted from Coleridge’s lecture of the previous week: “There can be no sincere cosmopolitan, who is not also a patriot” (101). Lawrence’s materialism was a political crime: in Coleridgean romantic circles, it was tantamount to treason against the state, as his physiological lectures had blasphemed against Church and Scripture.

The other – and in many ways more formidable – Coleridgean inside the College was J. H. Green, and it was his conservative romanticism that concerns us in connection with Owen. Green had been educated in Germany and apprenticed to his uncle Henry Cline at the RCS, where he received his diploma in 1815. He practised in Lincoln’s Inn Fields for twenty years. In 1816 he was appointed Demonstrator in Anatomy at St. Thomas’s. The following year, Coleridge visited Green’s house to meet the German philosopher Ludwig Tieck. (It was Tieck who prompted Green into taking the unusual step, at least for an English surgeon, of studying philosophy in Berlin (102).) So Green and Coleridge shared a common Germanising interest and they formed a deep and lasting friendship. Green attended Coleridge’s Thursday class (103), and became his amanuensis and medical disciple, while Coleridge himself included Green’s poems in his own collections. According to Trevor Levere, Green was familiar with Coleridge’s MSS “Theory of Life”, which he possibly helped revise while preparing his own lectures in 1824 (104). Certainly in a letter to Green in 1824, Coleridge alludes to their “confab” on Oken, polarity, the succession of animated nature, and the “*generic* in Thought” as contrasted to “*Genetic* in Nature” (105). And he mentions wanting to look over Cuvier’s works in the RCS library with Green.

In 1824 Green was appointed to the anatomy chair at the RCS and delivered his first twelve lectures on comparative anatomy – lectures which, Coleridge observed, “have deserved-

ly attracted much attention” (106). Owen was immensely impressed with Green (“that noble and great intellect” (107)) and attended most of the four-year course (1824-7), for which he provided accompanying dissections. Years later he recalled that

the first characteristic ... of this course – extended over 4 years – is that it embraced the entire range of the Science. For the first time in England the comparative Anatomy of the whole Animal Kingdom was described, and illustrated by such a series of enlarged and coloured diagrams as had never before been seen. The vast array of facts was linked by reference to the underlying Unity, as it had been advocated by Oken and Carus (108).

Carus’ “Comparative Anatomy” was the course text, and rather than take a limited teleological viewpoint Green concentrated on the “higher philosophy ... so far as the latter had been then based upon embryological and other researches”. As Owen concluded: “Green illustrated in this grand Course ... CARUS rather than HUNTER: the dawning philosophy of Anatomy in Germany, rather than the teleology which Abernethy and Carlisle had previously given as Hunterian...”.

Owen left telegraphic notes covering the last year (1827), which record Green’s class-room denunciation of Lamarckian “genetic” continuity. Undoubtedly, in Green’s *Naturphilosophie*, like Coleridge’s and Oken’s, the genetics thread running through life was really a Divine projection. There was no actual transmutatory continuity, as French materialists implied; no “Ouran Outang theology” as Coleridge

characterized the damnable philosophy peddled in Mechanics' Institutes (109).

Owen jotted during the lecture:

Considers nature as a series of evolutions – not under the idea that the lower can assume the characters of the higher – Lamarks [sic] (110)

Green was a chapter-and-verse Coleridgean, so Owen on his arrival in London was nurtured by Tory romantics for whom the “*plebification*” of science at workers’ institutes was plainly at odds with the medical priesthood’s role of making the lower orders more “soberly and steadily religious” (111). The teaching clerisy of the *Constitution* was to become the Tory vanguard against democratic reform and an antidote to the necessitarian, Utilitarian, industrial decay of national and religious culture. ‘Lawrencian’ materialism was denounced for its Continental socially-disruptive consequences, and Lamarckism deplored for its beastly contamination of the Divine thought. Owen we can assume was subtly schooled in the ideals of the medical clerisy and National Church; he attended Green’s lectures and dissected for him; and Green in 1832 acted as literary midwife, submitting Owen’s *Ornithorhynchus* paper to the RS, and recommended him for a FRS in 1834. From 1830-6 Green was Professor of Surgery at King’s College and opened the medical session in 1832 with an educational call in support of the National Church. As the bishop’s in the Strand had hoped to oppose the godless utilitarianism of Gower Street, so the *Constitution* was a conservative romantic response to Catholic Emancipation, working class reform, utilitarian educational ideas, cultural

“Spoliation”, and industrial contamination. Radical agitation and experimental systems of workers’ education had worsened the situation. In place of them Coleridge proposed

a permanent, nationalized, learned order, a national clerisy or church, [which] is an essential element of a rightly constituted nation, without which it wants the best security alike for its permanence and its progression; and for which neither tract societies nor conventicles, nor Lancasterian schools, nor mechanics’ institutions, nor lecture-bazaars [i.e. LU], nor all of these collectively, can be a substitute (112).

The idea was naturally anathema to radicals; Wakley deplored Green’s elitism, denouncing his “illiberality” and his “unparalleled insolence” to those he considered of inferior rank (e.g. apothecaries) (113). (For his part Green, as Sir Astley Cooper’s godson and Henry Cline’s nephew, co-signed an order banning Wakley from St. Thomas’s after his attack on the Cooper-clique’s nepotism.) Green’s and Coleridge’s national learned class was to comprise theologians, lawyers, and savants, to be educated *as a class* at university (meaning an ancient Established university); their function being to inculcate principles of religion, responsibility, and sobriety in the working classes. Morrell and Thackray have shown how strongly Coleridge’s vision appealed to the Anglican scientific elite of the ‘Cambridge Network’; but it was also taken up by the wealthy gentlemen surgeons and physicians in Lincoln’s Inn and Pall Mall. (The Oxbridge restriction for fellowship of the Royal College of Physicians ensured its conservative Anglican make-up.) Radical criticism

of the monopolistic regulations of these institutions, and of the nepotism and elitism of their officers – who were connected professionally *and* socially with the landed class with whom they shared social and educational ideals – constituted a direct *political* challenge to continuing Oxbridge Anglican hegemony. Green, and by association Owen, could only have treated it as an onslaught on the National Church and an attempt – parallelling parliamentary political moves – to shift the social balance of power in favour of the newly-enfranchised middle classes, while sensing the real threat to be *mass* participation. Since the managers of the RCS charged that utilitarian materialism fostered secular excesses and political instability, and since some of their radical critics had Lamarckian and French deistic tendencies, it is not surprising that “Ouran Outang theology” should have become a prominent conservative target in the 1830s.

The Political and Institutional Background: Lamarckism and Reform

Owen’s and Grant’s contrasting social self-perceptions and political aspirations are apparent from their respective social circles – circles which were separated by a political gulf in the Chartist years following the panic of 1832. Consider Owen first. We know that Oxbridge-educated members of the legal establishment were to play a key role in Coleridge’s clerisy; and Owen had begun developing ties with prominent Lincoln’s Inn lawyers while he was still practising

in Cook's Court. Among them was David Pollock (1780-1847), member of an eminent legal family, brother to George (later Field-Marshal Sir George) Pollock (1786-1872) and J. F. Pollock (1783-1870), with whom Owen was also to form a "lasting friendship" (114). J. F. Pollock became King's Counsel in 1827 and entered the unreformed House on the Tory side in 1831. (He was knighted in 1834 on becoming Attorney-General in Peel's first administration.) His son, who found Owen "a most wonderful and charming person" and regularly attended his lectures, often sitting alongside Bishop Wilberforce, recalled the gloom in his father's Tory circles at the prospect of reform in 1831, and the *Quarterly* talk in London coffee houses of the "approaching destruction of everything after the Reform Bill was carried": the prevalent fear that "The Church establishment and the House of Lords would go first, and the monarchy itself would soon follow" (115). David Pollock himself took silk in 1833 and was knighted in 1846 on becoming Chief Justice to the Supreme Court in Bombay. Owen actually carried a letter of introduction from David Pollock with him to Paris (116), which suggests that he was put in touch with conservative elements in the French capital. Of Owen's close legal, medical, and scientific friends in the 1830s whose names we can discover, many held Peelite views. Judicial Peelites – even Peel himself eventually – often took a close interest in Owen's science; indeed, in these pre-professional days many of the gentlemen were accomplished naturalists in their own right. Of none was this truer than of Owen's confidante and patron William John

Broderip (1789-1859). The Oriel-educated Broderip is best remembered for his penny pieces and zoological recreations; he also made crucial discoveries, particularly of the Stonesfield ‘opossum’, and kept a conchological cabinet in his chambers. Broderip was a frequent visitor to the Owen household. He wrote for the Tory *Quarterly*, contributed to the *Zoological Journal* and was a founding member of the ZS, being Owen’s backer in the Society. But much of this was by way of a gentlemanly avocation; what we tend to forget is that by *profession* he was a Thames Police magistrate, appointed to the post in 1822 by Lord Sidmouth, a high tory hated by radicals for his repressive measures (117).

Grant was obviously ideologically opposed to Abernethy and the other anti-‘Lawrencians’ at the RCS. In a sense he even stood on the opposite side of the law, defending the attacks made by the radical surgeon Thomas Wakley on the abuses of the medical profession – particularly on the nepotistic (118), self-perpetuating elite of the RCS, and on its monopolistic stranglehold on the profession. These attacks had brought Wakley ten libel suits, injunctions and legal actions in ten years – in some of which he was defended by Brougham. (Wasn’t Sir Anthony Carlisle referring to Wakley when he talked to Owen of the “active malevolence of ignorant savages”?* (119.) In its first ten years that scandalous

*Wakley had typically lambasted “Sir Anthony Oyster” (sic) for his 1826 Hunterian Oration, and as so often injected a

organ of medical reform *The Lancet* (founded by Wakley in 1823 with the help of Lawrence and working-class leader William Cobbett (121)) kept up an almost constant tirade against the “self-perpetuating, tyrannical council of the college of surgeons” labouring in “its sordid vocation (122). He campaigned ceaselessly for institutional reform to curb presidential and official privilege, for electoral rights for the rank-and-file members, and for increased access to the lectures, library, and museum. He demanded nothing short of a “radical reform of the mode of electing the College Council” (123) to check nepotism and privilege in the upper echelons and he demanded that certificates from the private schools be recognized, and not just those issued in public medical schools in which the council members themselves taught. In fact, his crusade for the latter proved a major success, showing, he boasted, the power of the free (i.e. radical) medical press – that “engine which has ever been the scourge of knaves, and the terror of fools” – when directed “against the despicable tyrants” (124). Wakley enjoyed great support

sacrilegious note, commenting that

whilst tearing asunder its bivalves [Carlisle was dissecting an oyster], lacerating its ligaments, and inflating its rectum, [he] piously observed, that the benevolence of an omnipotent power is exhibited in all the works of nature. Without questioning the propriety of this remark, we may, we think, be permitted to say, that it was at least ill timed, and we are inclined to believe, that had the oyster spoken, it would have given a flat denial of the Orator’s proposition (120).

Such effectively crude abuse and bitter sarcasm naturally galled the eminent surgeons, the more so since it concerned the sacred cow of Paleyism.

from the liberal wing of the profession in his fight for members' rights, judging by rising circulation figures and the collections taken up to pay his court damages.

Nonetheless his libellous attacks in the name of reform* and the free medical press brought him into headlong conflict with Owen's superiors at the RCS, particularly Carlisle (1768-1840), Sir Astley Cooper (1768-1841), who denounced

* In Wakley the union of the most strident elements of medical and social reform was complete. He had early struck a friendship with the radical editor of the *Political Register* William Cobbett; and Sprigge believes that Wakley's resolution to put his pen to propagandist use in the name of medical reform was as a result of Cobbett's influence and his own success in pamphleteering for electoral reform (125). With the backing of Cobbett, Joseph Hume, and the *ex officio* tactician of the reform movement, Francis Place, Wakley took his fight against corporate and clerical privilege to the House in 1835 as Radical MP for Finsbury. Here he was also allied on medical and social matters to Henry Warburton, crusader for the Anti-Corn Law League, postal reform, repeal of the newspaper tax, and, outside the House, the LU. Warburton supported Wakley's proposed National College of Medicine (see below), and in the Commons in 1827 fought for an examination of the reformers' grievances against the RCS; he also chaired a Select Committee on medical education in 1834. William Thomas in *The Philosophic Radicals* (1979) portrays Wakley as a minor but "eccentric" figure, one of the extreme 'doctrinaire' radicals "whose idealized picture of the poor drew its life from their hatred of the rich" (126). Wakley campaigned tirelessly against the Poor Law Amendment Act and sought a reprieve for the transported Tolpuddle Martyrs. Like all radicals he was committed to a total repeal of newspaper duties (an anti-aristocratic measure designed to bring newspapers within the reach of, and make them responsive to, the masses). Though he called himself a representative of labour (127) and was in favour of universal suffrage, secret ballot, and the abolition of property qualifications for MPs (and was present when the Charter was drawn up), he was not a Chartist himself, though he defended the movement and its leaders eloquently in the House. The parallel between his attempts to break aristocratic privilege and make institutions responsive to working people, and to democratize medicine and replace the obnoxious monopolies giving ordinary members a greater say in the running of their colleges, is obvious.

the “Reptile Press” and Abernethy, who sought an injunction to stop Wakley pirating and publishing his College lectures. (This was denied by Lord Chancellor Eldon, in what was taken as a victory for the radical press (128). It also resulted in *The Lancet*’s circulation figures topping 4000 a week in 1826-7.) Owen was employed by the RCS. He owed his position to Abernethy, and was encouraged and supported by Astley Cooper and Carlisle (Presidents of the College in 1827 and 1829 respectively, and physicians to George IV). Indeed, the very *reason* he was brought in as assistant conservator to Clift was to overcome one of the reformers main charges: Wakley and James Wardrop (“Brutus” of the *Lancet*) highlighted the frustration of members trying to gain access to the library and museum, which were subject to restricted opening hours, or to see Hunter’s MSS and preparations, purchased out of public funds and housed in the Hunterian Museum. The Fellows “take our money, give us *ex post facto* laws, lock up our property, insult us with mock orations, live at our expense”, raged Wardrop in 1825 – yet the “commonalty” is as “entitled to the museum and the property of the College as any member of the Court”:

some years ago Sir William Blizzard promised that the library should soon be opened. We are still outside the door, however, and a part of the bust of Sir William, like the molten calf of the Israelites, may go down our throats (by the adulteration of bread or in the drinking of soda water) before we shall see a book, especially the Hunterian MSS

When Wakley, Lawrence, Hume and twelve hundred others met in

the Freemasons' Tavern in 1826 to petition parliament over this disgraceful state of affairs, and Warburton took up the case inside the House, Abernethy announced that the reason for the members' non-admittance was the absence of a catalogue, destruction of many of Hunter's MSS by Home, and the poor condition of the library. He then hired his own protégé Owen at £30 a quarter to catalogue the collection, creating his assistant's job as part of an overall strategy to pre-empt radical criticism. Owen was subsequently groomed by Abernethy, Carlisle, and Cooper, and initiated into Coleridgean 'higher anatomy' by another Wakley antagonist, J. H. Green. Under these circumstances, prudence, if not Owen's own sense of disgust at the scurrilous democratic attacks on the institution, would have dictated his condemnation of *The Lancet*. Anything less would have been incompatible with his initiation into the charmed circle and his friendship with the legal and medical gentlemen of the Inn.

The attraction of Grant's science for Wakley was manifest. It was naturalistic and secular; it developed principles formulated by Parisian philosophic deists and paid no heed to the cruder kinds of teleology by which "Church and State' bigots" sanctioned their privileged position. Probably Wakley only became fully aware of the ideological strengths of Grant's naturalistic science on publishing his "brilliant" course of LU lectures; at least, he declared after finishing them that he was astounded at the "depth and extent" of Grant's understanding (130). Thereafter he lost no

opportunity to promote the Gower Street professor: publicizing his activities (131), supporting (indeed *suggesting*) his candidacy for vacant chairs (132), smiting those who would stand in his way (including Owen), and generally ‘puffing’ in the best tradition. As a result, the radical *Lancet* is one of our chief sources of knowledge concerning Grant’s activities in the 1830s. In a sense it provided him with a propagandist vehicle, if not a power base. Wakley considered that Grant’s lectures would “for years stand unique for arrangement and details” (133); indeed, in an effort to secure the Physiology Chair at LU for Grant after Quain’s retirement in 1836, Wakley’s puffing reached epic proportions. He extolled “the brilliant genius of Professor GRANT” and claimed that his “acquirements in physiology, – the highest department of medical science, are not surpassed by those of any professor in Europe” (134).

Grant’s secular science was an integral, but not necessarily primary, reason for Wakley’s frantic canvassing. It had actually been Grant’s strident call for medical reform and criticism of the monopolists that had first attracted Wakley’s attention. Though a Fellow of the Royal College of Physicians of Edinburgh (and thus entitled to practise in Scotland), Grant was legally barred from practising in London without first submitting to a RCP examination. This he refused to do, joining the angry chorus in denouncing the College’s monopolistic stranglehold on the profession,

willingly risking his professional and financial prospects for the sake of the principle.

The iniquity of the system was not lost on visiting foreigners; Professor K. F. H.

Marx in his observations on English science wrote:

Dr. Robert Grant is a modest Scotchman; following science quietly, and devoting his whole time to his own pursuits. He is a physician but does not practise, or rather, must not; since he refuses to comply with the strange regulations of the College of Physicians, which require him to pay handsomely for the permission to practise, although he got that permission long since at Edinburgh (135).

In his address at the opening of the medical school in 1833 Grant deplored the RCP's exercise of "exclusive power" over the profession as "injurious to its interest, and an unjust oppression", while characterizing its Oxbridge degree requirement for Fellowship as a perversion. Since to obtain degrees at the "imperfect" Oxford and Cambridge medical schools students must sign the Thirty-Nine Articles, RCP Fellowship was restricted to wealthy Anglicans. He therefore welcomed the setting up of a Commons committee to examine "the existing evils" of the medical system (136). He rounded off in tones that Wakley would have approved:

The busy hand of reform, everywhere aiding reason against custom and power in establishing the rights of man, is nowhere more wanted than in the Medical Institutions of our country, to adapt them to the present wants and condition of society. The exclusive and obnoxious power gradually usurped by the College of Physicians of London, is contrary to reason, justice, expediency, and public good. The system of Apprenticeships to Apothecaries and Surgeons is a system of menial occupation, idleness or vice; it is a remnant of the low and ignorant state of the profession in olden times; it serves only to secure pecuniary advantages and gratuitous service

to a few interested men...(137).

He denounced the absurd regulations of the “chartered Companies”, arguing that a reform of the law must limit their privileges, which were proving so “ruinous” to the progress of medical education. It must also establish examination boards independent of vested company interests:

As it is obviously the interest of every teacher to circumscribe the curriculum as much as possible within the limits of his own department, it is as injudicious to leave to them the planning of that curriculum, as it is to make them also the sole judges of the qualifications of their own pupils for the highest honours in Medicine it tends only to corruption, to make them a mart for the traffic of distinctions, and a scene of mercenary contentions, however expedient it may be for the State to enrich them with the profits arising from the sale of honours (138).

He decried all “adventitious privileges and dignities” as “absurd vanities” and wanted no exclusive rights bestowed on the unendowed LU – rights he considered divisive which could only lead to further “inequalities of rank”, protectionism, sectarianism and disintegration in the profession. Holding to its utilitarianism the university would supersede the restrictive chartered companies by diversifying medicine, placing it within a scheme of liberal education, and protecting its higher and less popular “departments of knowledge”.

The Lancet reported that applause for Grant’s lecture was the loudest “ever heard within the walls of a medical institution”. The liberal press responded favourably. The *Medico-Chirurgical Review* thought that the RCP was “truly

represented as not only void of all utility to the profession at large, but as injurious, unjust, and oppressive” (139). Speaking for the radicals Wakley recommended Grant’s “brilliant essay” on the inequalities of “medical government”; and in his first public recognition of Grant wrote: “it proves that the professor is a man not only endowed with extraordinary mental powers, but that his moral courage, in declaring his purposes, is equal to the knowledge with which he is prepared to press them on the attention of the legislature and the country” (140). Grant reciprocated and in class praised “Mr. WAKLEY, the indefatigable and learned editor of ... THE LANCET ... a castigator of evil-doers, and a rewarder of the good” (141). This mutual back-slapping infuriated the conservative press, particularly the *London Medical Gazette*. This had been founded in 1827 to counter *The Lancet*’s pernicious radicalism; it was designed to unite “those members of our profession who have its respectability at heart” and were “exposed to attacks” from the radical press (142). It made a direct appeal in its first number to those who viewed medicine “as an honourable source of maintenance and respectability in society, who wish to see it followed and practised with philosophic views, and gentlemanlike feeling...”. Wakley routinely derided the *Gazette*’s layout as a “contemptible imitation” of *The Lancet*, and its contents as an “out-pouring of vulgar and malignant scurrility” (143). Nonetheless it had political importance for establishment surgeons. Charles Bell, shortly to be knighted,

felt honour bound to support it, commenting on the feeling of “the more eminent and respectable of the profession that it is meritorious to give it countenance and assistance” (144). The *Gazette* grudgingly acknowledged that Grant’s lecture, whatever its “besetting sin of grandiloquence”, was not as bad as it might have been (145); indeed, by a piece of judicious misreading the following week the reviewer could even agree with Grant’s sentiment that the joint-stock university should survive on merit and not privilege implying that it should be denied a charter (146).*

* Whatever the reformers’ idealized conceptions, it was never a debate over the demolition of all privilege, but a conflict over the redistribution of privilege in consequence of new political realities. As conservatives were aware, reformers complaining of the entrenched power of the RCS and RCP were simultaneously lobbying parliament through radical MPs like Tooke and Warburton for a charter allowing LU to grant degrees, something which would give the university itself an advantage over the rival medical schools and corporations. That they got their charter reflected the growing power of middle class dissenters, utilitarians etc and their representatives in the reformed House. Radicals like the general practitioner George Webster announced, after the charter had been granted enabling degrees to be conferred “without reference to any religious opinion or distinction”, that the University only wanted the power to license those whom it examined in order to equal and “eventually supersede, the medical corporations” (147). The *Gazette*, as mouthpiece of the corporations, remained implacably opposed to the utilitarian joint-stock school. It repeatedly expressed barely-concealed class fears – that the mercantilist proprietors were intent on seizing an increasing share of corporate medical power. Its readers defended the RCP’s maintenance of “an order of practitioners [i.e. Oxbridge-trained fellows], educated in the same manner and in the same classes as the highest rank of society”, despite demands for equal status by the Presbyterian and Dissenting Licentiates and in defiance of the reformers’ petitioning of parliament to demolish religious and class distinctions and throw open the fellowship to all practising physicians (148).

Grant's support for Wakley, was another matter. The *Gazette* deplored this reciprocal puffing between a professor employed in a joint-stock school and "the radical portion of the medical press" (149). Particularly it was worried by Grant recommending a radical press to his pupils. In a slamming indictment, the writer asked what part of "his patron's exertions" Grant would recommend most to his students: his "insolent mockery" of eminent surgeons, his attempts to suppress the Anglican school in the Strand; or his "treatment of Abernethy, Astley Cooper, or Sir W. Blizzard?". In the critic's eyes 'radical' was synonymous with 'criminal':

Perhaps he will also point out to his pupils what part of Mr. Wakley's conduct he considers as chiefly meriting their applause; – what portion of his career, from his first attracting attention, in consequence of the destruction of his house by a still undetected incendiary [150], to his *fourth* conviction for libel in a court of justice, he would especially recommend to their imitation.

The only charitable explanation, sir, of all this, is that Dr. Grant did not *know* the character of the publication of which he spoke; I therefore take leave to inform him that he has expressed his approbation of a paper *which has existed by slandering his brethren – which has systematically dealt in libel and falsehood – which has violated every principle of professional etiquette and honour – and which has not scrupled, when it served the purpose of the moment to have recourse to forgery* – aye, sir, FORGERY is the word: to which long list of literary virtues, I have to add, that of ribald jesting on holy things, and blasphemous derision of the sacred truths of Christianity! Such are a few of the characteristics of the publication which Dr. Grant has represented as the "rewarder of the good" – SLANDER, FALSEHOOD, FORGERY, and BLASPHEMY! and I leave it for others to decide whether these be qualities which entitle a work to the praise of those entrusted with the care of public education, and holding the responsible situation of the instructors and the guides of youth (151).

Other teachers were similarly attacked for their radical associations. The *Gazette* had already denounced the Professor of Midwifery D. D. Davis (1777-1841) for shamelessly inviting Wakley – “that virtuous monitor of youth, and epitome of moral graces” – to a LU *soirée* (where, to the *Gazette*’s unutterable disgust, he was warmly received (152)). Nor did it approve his support for Wakley’s proposed London College of Medicine in 1831 (a socially-levelling institution in which all who were licensed to practise, from whatever school, were to be admitted as Fellows, and all Fellows, whether surgeons, physicians, or apothecaries, were to carry the title Doctor – an institution to be run on democratic lines, with officers and senate elected by annual ballot (153)). But this was nothing compared to the contumely heaped on Grant for approving in class a journal that dealt in libel, slander, and blasphemy. Wakley was well pleased by Grant’s subsequent “condemnation, masculine and powerful” of the “Paltry slanderer” (154). As well he might have been, for Grant used Wakley’s own brand of fiery rhetoric to lash those “captious hirelings” and “impudent menials” who would question the editor’s character, and he ended on a rousing defence of *The Lancet*’s record in fighting corruption, monopoly, and private interest (155). That conservative feeling was running high against Grant was evident from the *Gazette*’s warning. It never doubted his scientific ability or competence in comparative anatomy (156). Nonetheless it persisted in warning him that he could only injure himself by siding with the violators of public decency. No naturalist

could maintain any “pretensions to respectability” while openly abetting “the conductor of a publication which sets all morality, courtesy, and even decency, at defiance, which outrages every feeling acknowledged among gentlemen, and violates every principle held sacred in society” (157).

Knowing the establishment backlash against Grant, we can place Owen’s response in a broader social context. He was not merely a passive *employee* of the besieged RCS, but was nurtured and guided by Carlisle, Abernethy, Astley Cooper, and Green. Nor could he have remained indifferent to the political crisis or his superiors’ expectations, had he wanted to. He was not only socially *ensconced* with eminent surgeons whose social views he would have espoused; but as the debate progressed his own department in the College came under direct political attack from Grant *qua* radical reformer.

In 1841 Grant delivered a passionate annual oration before the British Medical Association, a “bold and daring” address, wrote Wakley, which “grapples lustily with the gigantic evils that oppress us” (158). The short-lived BMA (f. 1836) represented the London general practitioners’ attempt to unionize in opposition to the Poor Law Commissioners (159), although through its leadership (particularly George Webster) the union came to espouse wider radical aims: the establishment of democratic medical government under one faculty, destruction of all “degrading distinctions” and titles, and

abrogation of Corporation privilege. As a result it quickly adopted a confrontationist stance towards the plumed aristocrats of the RCP and self-electing Council of “the most illiberal corporation in the kingdom”, the RCS (160). Grant’s speech to the Association in 1841 was as strident as any Wakley or Webster ever made: a bitter satirical attack (running to 98 pages in print) on the corrupting influence of corporations, coupled with a personal condemnation of the dehumanizing illegal actions of the RCP in suspending his own professional privileges. He berated the “shop keeper legislature” for ridding itself of its obligations by sub-contracting medical jurisdiction to the corporations, whose Councils, by a corrupt misuse of power to ensure perpetual self election, had infringed the “common rights of a free people”. Each corporation had degenerated into “a kind of commercial firm possessing a patent or monopoly in a little article of traffic [i.e. in diplomas]”, allowing aggrandizement of corrupt officials where profits should be returned to the profession. His remedy was for the State to support and finance medicine, as on the Continent, and for administrative control to rest with the Secretary in the Home Department. Grant’s political punch, though lacking Wakley’s vituperation, was all the same a bruising mixture of solicitation, satire, and invective. The radicals’ use of political metaphor was meant to remind the legislature of events across the Channel: Webster talked of apothecaries as the “third estate in the realm” (161), and Grant of the dissatisfied lower orders as the “*canaille* of Licentiates”.

But mostly Grant's metaphors for reform were taken from nature or medicine. He argued that incorporation by Charter sanctifies vested interest and protects it "from the innovations of reason and the inroads of improvement". Corporations have thus sunk to "conservatories of bigotry and ignorance", concerned only to protect their monopolies. As these chartered bodies constitute

part of the organization of the State, and were made for public, not private advantage, they must be preserved fit for their functions, or removed as morbid excrescences. The most effective and fundamental changes in a torpid constitution, are effected, not by spontaneous action, but by foreign agents, through the 'prima viae,' which produces a healthful re-action to remove the distemper; or, by the weakness of the *vis medicatrix*, induce the more violent, but more curative paroxysm of a radical Reform.

He urged the Treasury to take over financing, abolish grades, and institute a uniformity of rank, privilege, and education under a General Governing board. Electoral representation, annual ballot for managerial posts, by-laws approved by the membership – all these had been won by the learned societies. So in medicine free competition should prevail, opening posts to talent rather than leaving them dispensed as patronage. His greatest anger was reserved for the corporations, those "hot-beds for the development of all the higher vices". The RCP appeared to him determined to "crush dissenters, or Scotch Graduates ... and to forward the interests of the English Church", by creating *religious* and class distinctions between the Oxbridge Fellows and non-

Anglican Licentiates (whose dissenting/Presbyterian case was of course championed at the university). By-law juggling had enabled the RCP to become a “kind of aristocratic high Church Establishment” engaged in the “public fraud” of extracting money from those already licenced – a fraud Grant refused to condone, declining to “disgrace” his existing Fellowship of the RCPE by submitting to the college’s “arbitrary, illegal and ignominious” demand for re-testing (162).

Some of Grant’s most biting criticisms were reserved for the self-elect of Lincoln’s Inn, whose greed and traffic in diplomas had led them to ignore the unprofitable zoological and pathological sciences. This aspect of the RCS targeted by Grant is revealing. He made a digression into the failure of *comparative anatomy* at the college, and his shafts, if not *aimed* at Owen (who as Hunterian Professor held responsibility for the science), would have severely wounded him. Grant considered the Hunterian Museum’s authority actually the greatest “impediment” to the progress of comparative and pathological anatomy in the country – with any “affected encouragement ... more calculated to insult than to promote” the sciences. The Lincoln’s Inn museum like its counterparts in Edinburgh and Dublin actually proved detrimental to the “diffusion of knowledge”, since it obstructed the formation of rival collections in universities and private schools (163). The politically-motivated attacks of Grant, Hall, Davis, Wakley, and other university sympathisers at once reflected and deepened the alienation between the competing

Gower Street and Lincoln's Inn schools and at a time of heightened social tensions when their ideological split translated into grave political differences. A romantic conservative like Owen educated in the ways of the National Church would have seen the criminality in rabble-rousing attempts to upturn the social order; while radical attacks on his superiors (of whom he was obviously fond) could have incensed a young Peelite with social pretensions, making a scientific response professionally expedient. Were none of this true, Grant's criticizing his department's would anyway have made neutrality impossible for Owen.

Within this socio-political compass certain aspects, like Grant's naturalistic science and anti-Providentialism – the ideology of doctrinaire radicalism – would have infuriated Owen. Grant's transformism (which Owen singled out from the *Lancet* lectures (164)) would have been seen as a function of his mechanistic and reductionist philosophy – the sort which Abernethy had already castigated as socially irresponsible. In opposition to Abernethy, the vitalists, and the romantics, Grant was committed to physico-chemical explanation for all organic phenomena. He sought no further than unity of composition and the laws of animal development for organic explanation, while failing to place this in a natural theological context. Grant's transformist morphology was obviously wide open to attack from conservative quarters: an attack that would have been interpreted in the Council

chamber of the RCS as a *responsible* reaction to an obnoxious socially-levelling religious and political threat.

Grant's naturalistic science and secularism were by Owen's gentlemanly standards extreme, however typical of the democratic labour-orientated wing of the radical movement. With the corporations allied to the governing classes, and Anglicanism constitutionally privileged, Grant's characterization of the RCP as the "tool of a high Church party" (165) was in one aspect a doctrinal threat to the physicians, just as his secular biology taught at the "Godless College" (166) was a *religious* one to the surgeons.

The anti-clerical Paineite strain of the working class Reform movement and its reflection in Grant's godless biology would naturally have concerned Anglican romantics. Both Grant and Wakley had a cruel wit on the subject of Christianity. Time and again the *Gazette* denounced Wakley's "scurrilous jibes" at Scripture and accused him of the vilest blasphemy (167). Wakley satirized Jonah's residence in the whale for political ends, and Christ's miracle of the loaves and fishes "which some of the profane 'band of modern sceptics' have had the audacity and folly to deny" (168). And of course he was notoriously anti-sabbatical in the working-class cause; as when he opposed the Lord's Day Observance Bill in 1836 on the grounds that working men only had Sunday free to spend their wages (taking the pragmatic line that Sunday closure would push them into public houses on a Saturday night to spend

their money) (169). Later in life, at least, Grant too adopted a sceptical and mocking tone in class. Rickman J. Godlee recalled that “we students used to listen with a sort of guilty pleasure to his satirical references to Providence and other matters then almost regarded as too sacred for enquiry” (170).

Whereas Grant in his lectures refused to consider man as anything but the highest animal, Owen as an Anglican functionary in an elite corporation saw anatomical science underpinning natural theology. Even his lifelong interest in apes – begun in 1830 – was relevant in a higher sense because it was only by comparing human and ape anatomy that we can “possess the true means of appreciating those modifications by which a material organism is especially adapted to become the seat and instrument of a rational and responsible soul” (171). This goal was to sustain him for thirty years; from 1835 to the 1860s (the time of the “hippocampus” debate (172)) he believed that he could effect a final *anatomical* dissociation of man from ape. Owen was never as latitudinarian as Baden Powell, and was somewhere to the right of Charles Babbage; but like them he accepted a universe of wise design and creative foresight. In his Hunterian lectures for 1837 (read before an audience including Sir Astley Cooper and William Buckland) Owen used comparative anatomy to elucidate man’s relationship to the Almighty. Science had taught man

that if he were something less than a Deity, he was

something more than dust; – it appears to have been essential that he should know that an intelligence was superadded to matter, and had presided over its arrangements, but that the Omnipotence to whose Fiat both he and matter owed their existence, had in this world endowed him alone with faculties to appreciate the Works around him; and would, in another, require from him a strict account of their Application (173).

We know that from the later 1830s he was to subordinate Paleyite teleology to a theodicy of Archetypal design and by the late 1840s was making a tentative appeal to law. Even then he was careful to interpret secondary causes as Divine ‘ministers’, while utterly repudiating the Puseyites’ charge of pantheism (174). As early as 1832, six months after his return from Paris, he was musing in his notebook on the use of the term ‘Nature’ to signify God’s Works, and the self delusion in imagining that this subsumed the moral dimension, or that from ‘Nature’ alone we could advance to the First Cause (175). Owen took his religious duty seriously, and his convictions are quite apparent from his instructions to Clift, left in temporary charge of Owen’s son Willie

Whilst our dear boy is with you you have a dear and deep trust. Every seed now sown will spring up for good or for evil. His only happiness and our’s depends upon his truly fearing & reverencing God and his holy Word & Laws. Guide him to that by example. Let the short time that is allotted to us be spent, as far as we are able, in showing our gratitude to the Heavenly giver of our peace and competence and the blessings of this life, which, if we look around us, we cannot but see to have been abundantly bestowed. “It is He that hath made us and not we ourselves.” It is He, too, who teaches and guides us to good, if we look with a humble and teachable mind into the Volume of His Precepts (176).

Radical Godlessness would have seemed to Owen educationally irresponsible, politically disastrous, and a measure of the social abandon of the extreme reformers. His larger responsibilities as an Anglican anatomist would have demanded an effective gentlemanly reply, which – since it was his *professional* mode of expression – meant a *scientific* reply.

The Professional and Personal Dimension

The Deterioration of Relations

Although the evidence is scanty, it is plain that by 1840 Owen's and Grant's personal relations had deteriorated, providing scope for a study of the way ideological rifts can translate into personal actions. The shift from ideology to action must be mediated at innumerable professional and social levels, and the importance of this professional level in particular should not be underestimated. Competition for the few available posts was keen in the 1830s, and professional zoologists vied for them to supplement insufficient salaries. Grant rose smartly in the early 1830s, and not for nothing did Edward Turner dub him the future “*Cuvier of this country*” in 1831 (177) – a full eighteen years before Owen was visited with the title. Grant was elected onto the Linnean (1829), Geological (1832) and Zoological Society (1833) Councils in quick succession. Thus it could be argued – if one uncritically accepts Huxley's (later) personality assessments – that Owen would have been urgently pinning ‘no poachers’ notices around his preserve in

the 1830s. His idiosyncratic traits, so successfully harped on by the Darwinians, might be employed to explain his need to combat Grant. But extreme caution is needed in this approach: Huxley's perceptions carried their own ideological distortion (178), and I doubt the propriety of extrapolating back two decades, to a time when Owen was less secure and possibly displayed less of the "cock of the walk" manner that so infuriated Darwin (179). Personality assessments enable us to detail the fine grain of science history but beyond that often permit only a shallow level of interpretation, and in many cases the deeper social and political currents can be used to set an actor's behaviour against the cultural expectations of the age in a more satisfying way.

The main reason why the professional dimension should not be ignored is that Owen's rivalry with Grant for available resources (i.e. dissection material) and jobs would have been heightened in an age when both were in short supply. Limited teaching arrangements in the hospitals and institutions (180) meant that they competed in the same badly-paying field. Neither man was independently wealthy, nor earned a salary commensurate with the needs of a gentleman. We have seen that Grant's share of his fees between 1830 and 1835 fluctuated between £66 and £111 p.a. Owen fared rather better; but even his £300 p.a. by 1833 was deemed insufficient to contemplate marriage (181). As a result both searched out additional lecturing posts. Grant taught at the medical schools of Great Windmill Street and Aldersgate Street, and delivered

physiology lectures at his home in Euston Square (182). After unsuccessfully applying for a number of posts (183), Owen was eventually elected to the newly-established Chair of Comparative Anatomy at St. Bartholomew's Hospital (holding it concurrently with his conservatorship at the RCS). It was inevitable that they should compete directly on occasions. In 1831 Owen was a candidate for Charles Bell's Chair of Physiology at LU (184), which Edward Turner thought should go to Grant (185). (One can appreciate the competition – this was a rich chair.) And Grant only got the Fullerian Professorship at the RI in 1837 after Owen (the Managers' first choice) turned it down (186). Again this seat was hotly contested, not only because it paid moderately well for little work (£50 half yearly, (187)), but because it was prestigious and the RI attracted large fashionable audiences: thus some of the best men put up their names in opposition to Grant and Owen (188). The RCS Council evidently forbade Owen to accept "any other office" until the Hunterian catalogues were complete (189), whereupon he declined and Grant was "unanimously elected" (190).

Professional conflict might also have erupted at the ZS, where both men held Council seats (from 1833) and Committee appointments, and both worked on the Society's precious resources. We have seen that all manner of reformers (including Grant, Swainson, and Bicheno) were trying to make the Society more scientific and its managers more accountable

(in accordance with Reform principles). Thus this institution provides a test-bed for estimating the way Owen's ideological bent manifested in practice – in his actions to support the social status quo. Clearly the Society was of central concern to both men, but they obviously envisaged it moving in different political directions. Both were prominent in its administrative affairs: Grant sat on the Committee of Science and Correspondence in 1830; Owen (FZS 1830) joined the Council in 1832, Grant a year later; both sat on the Publication Committee (1833) and Museum Committee (1834) (191). Grant, and more so Owen, published extensively on Society material; there was a natural division, with Grant concentrating on invertebrates (especially cephalopods) and Owen on mostly mammalian vertebrates (he published an astonishing fifty or so papers on ZS specimens from 1831-5). Britain's expanding colonial interests made this a rich source of supply, and natural history being fashionable at home (192) there was a constant demand for lectures on exotic beasts. But the rumbles of reformist discontent were ever present, and as Wakley said, the Society was flourishing “in spite of the bad management” (193). So the wealthy ZS was a potential source of immense power, but was suffering internal division, with reformers trying to democratize its membership in opposition to the gentrified Tory backers. Here if anywhere we should be able to plot the shifting power-base of the London Lamarckian and tackle the way Owen's Peelite politics translated into action.

In chapter three we discussed at length how a Tory junto in the Council effected the dismissal of Grant and others in 1835, while resisting pressure for the officers themselves to be subject to electoral removal. This of course infuriated a democrat like Wakley, and Grant was always to remain bitter. Administrative criticism (itself *political*) was the direct cause of the conflict leading up to the election rout; nonetheless it has been a leit motif of this thesis that radical ideology translated into both political action and reformist secular science, and there can be no denying that Grant's pointedly materialistic zoology would have been a determining factor in the junto's action. We know that in January 1833 Grant began delivering a major course to the Fellows. The Society's museum in Bruton Street was packed to overflowing (194), and Owen's own *Zoological Magazine* reported that the opening lectures gave "great satisfaction" (195). It is difficult to tell from the 19-page prospectus whether the course was any less materialistic than its university equivalent. There is little reason to assume that it was. Transformism may well have been mooted since (as in his LU prospectus) he promised to look into the origin, limits, and duration of species, and the changes effected by environment, domestication, etc. He covered precisely the themes discussed at Gower Street: "unity of plan" throughout the *whole* animal kingdom, "gradual development" of animal forms, imperceptible gradations in the chain, and recapitulatory confirmation of ascending development (196). Again

beginning in January 1834 he delivered ten lectures in Bruton Street on “Fossil Zoology”, in which he is likely to have urged the continuous serial ascent of fossil life (197), although to what extent he broached cosmological factors, cooling earth dynamics, or Lamarckian generation is simply not known. Nonetheless the fellows’ exposure to his thought was lengthy enough to lend plausibility to the idea that its *totality* – scientific, political, and religious – would have rendered him triply obnoxious to Tory managers.

Even the subjects he was known to have broached would have given Owen cause for concern. Both in his Bruton Street lectures and papers published in the ZS *Proceedings*, Grant tackled a favourite theme: the analogy of naked cephalopods and cyclostome fishes (198). By positing a rudimentary vertebral column and “appendicular” skeleton in cephalopod molluscs, he was explicitly endorsing Geoffroy’s transition between Cuvierian *embranchements* (and underpinning a transformist continuum). Owen by contrast weighed into the Geoffroy-Cuvier debate on the opposite side. In one of his earliest memoirs, in 1832, he used the Hunterian Museum’s newly-acquired pearly nautilus to argue against “the theory of the simple and unbroken series” and Geoffroy’s “modern” doctrine of intra-divisional “unity”, even though “advocates of the doctrine ... have endeavoured to produce a semblance of conformity between the *Cephalopoda* and the *Vertebrata*” (199). So from the first Owen took up a diametric *scientific* stance. It is probably no coincidence that his first public anti-

transmutatory statement, with its lengthy, refutation of Bory, Lamarck, and Geoffroy on the question of the ape's transformation into man, was ready for the ZS's *Transactions* in May 1835 (200), the month Grant was balloted off the Council. By this time Owen was voting in the party interest. He backed Broderip, Sabine, and the junto in recommending Grant's removal, causing Wakley, on hearing in Benjamin Brodie's Hunterian Oration of Owen's succession to the Hunterian chair, to remonstrate:

It would be well if it were made known to what extent the powers of Mr. Owen over the contents of the museum in which he is placed with this great privilege, are to be exercised. Those supporters of science in this metropolis who are acquainted with the fact, that when he had the opportunity of supporting the claims of Professor Grant to a post of honour and eminent usefulness as a comparative anatomist and physiologist, in a late much-discussed election at the Zoological Society of London, Mr. Owen voted against the appointment to office of his eminent brother physiologist – to Mr. Owen's eternal disgrace (201).

Owen's anti-transformist stand and support for radical-combatting Tories at the ZS was perfectly consistent with his election to the Hunterian chair at the RCS. 1835-6 marked the height of radical agitation at the ZS. On his return to England Darwin was appalled at the bickering and refused to take much part in the ZS's proceedings on that account, opting for the more tranquil GS (202). Owen and Broderip in 1836 gave short shrift to the remaining "malcontents", decrying the "jaundiced eye with which some – they are not many – look upon everything belonging to the Society", and

declaring that the constant squabbling “bodes the Society no good” (203).

The junto’s machinations and the ensuing bitterness stopped Grant from taking any further interest in the Society (204). With the reformers either gone or silenced, the conservatives regained the whip hand. Grant effectively left the field to Owen, and in so doing lost a major platform and source of funding; but more importantly he lost access to valuable dissection material. Owen on the other hand went from strength to strength. By 1835 he was already monopolizing cadavers, such that the Secretary E. T. Bennett was forced to approach *him* whenever others requested specimens. Typical was Bennett’s plea:

Tiedemann is anxious to see all the brains we have. You have more brains than most of us I know, and will advantage him by them. But those I am now thinking of are not in your *cranium*, but such as have been removed from the *crania* of our animals (205).

From 1836-40 Owen published over thirty-five more papers on the Society’s material. By the end of that time the Council, backed by Owen’s allies like the Tory MP Sir Philip Egerton and Lord Braybrook, ordered that he be “allowed to dissect whenever and whatever he liked when death occurred at the Gardens, and that he is to have precedence over any other person” (206). After this, anatomists often tackled Owen directly if they needed access to Society material – as in the case of the aural surgeon Joseph Toynbee (1815-1866),

Owen's one-time assistant at the RCS, now preparing his famous work on articular cartilage. In 1841 he requested *Owen*'s permission to dissect "the diseased joints of animals that die at the Zoological Gardens" (207), the sort of request which suggests that Owen was considerably more powerful than his titular status might imply.

We might guess that personal relations would have deteriorated as Grant began to lose his power base, thereby impairing his ability to research and publish effectively. Indeed, knowing Grant's uncompromising radicalism and harsh naturalism, and Owen's social ambitions and conservative Anglicism, this might have seemed inevitable. On issues of personal morality, too, there could have been profound disagreements. The bachelor Grant is suspected of having been homosexual (208); although we have to tread most carefully here – testimony passed down by word of mouth in Grant's own department is not perhaps the best basis on which to rest any judgment. If he did, as is suspected, openly practise, then one can speculate that respectable family men like Owen and Huxley would have viewed it as an abuse of his teaching position. But any real evidence is meagre to the point of non-existence. One could ask, perhaps, what Edward Forbes meant by asking Huxley in 1852 whether Grant had manifested any new "eccentricity"? Since Forbes was not unduly perturbed by Chambers' *Vestiges* (in fact he thought it like "a breath of fresh air to workmen in a crowded factory", for all its faults (209)), he was probably not referring to Grant's

transformism, leaving one to wonder whether he did not in fact mean eccentricities of a more personal kind. Such indeed might help to explain Huxley's own reaction. One might have imagined that the secularist would have rewritten history to reinstate the materialist evolutionists. But he failed to treat Grant kindly, considering that his advocacy of evolution "was not calculated to advance the cause" (210). The new pantheon was to elude Grant; Huxley, a respectable family man, might have seen him as much a social reprobate as Owen had done. (There are, as we have seen in Ch. 1, plausible counter explanations for Huxley's move, relating o Grant's cynical materialism and the structure of his science.) Nonetheless, Grant's suspected homosexuality is a problem compounding any interpretation of his social predicament. No "social experimenter", as Irvine calls Owen (211), could reasonably expect to retain him as a friend and move in more elevated circles. And had Grant's behaviour been suspect, it would have done little to restore Owen's faith in the moral advancement of man promised by reformers in their meliorist utopia.

So we have grounds for believing that Owen's social and ideological grievances, realized at a professional and societal level, made any lingering friendship impossible. Exactly when relations first cooled is difficult to say, but by 1841 they had degenerated into open hostility. Niggling and otherwise unimportant issues blew up into questions of

personal veracity. Indicative was the dispute in April-May 1841 over the ownership of a case of zoological specimens shipped from Van Diemans Land by Edmund Hobson. Hobson was Grant's pupil and medallist, and had already donated skeletons received from the colony to the zoology museum while at LU in 1837 (212). Before sailing to Tasmania, he also tackled Owen on its zoology and promised him specimens (213). Unaccountably, his first shipment to Grant in 1841 was mysteriously readdressed *en route* and ended up at the RCS with Owen, who laid claim to it, causing a long acrimonious dispute only resolved in Grant's favour when he produced a witness who had actually been present at the packing and another who testified to a switching of the labels aboard ship (214). (Owen, like many palaeontologists, was dogged by priority and ownership disputes (215), and protagonists laid the blame for their frequent bitterness on his cold personal manner. This seems to have been one of the earliest of his ungentlemanly brushes.) More important were the heated public exchanges with Grant at the GS over the interpretation of key fossils, for instance Koch's Mastodon (1842), and more important the Stonesfield 'opossum' (1838-9), where the correct interpretation was of crucial ideological importance (discussed fully in Ch. 7).

So it seems profitable to interpret Owen's and Grant's differences from a contextual point of view, as symptomatic of a deep-seated ideological divide, while trying not to lose sight of personal idiosyncrasies on both sides, which must be

adjusted for in any rounded explanation. With Grant supporting reformers laying siege to the RCS, Owen would have been only too aware of the social, professional, and political threat in this age of crisis – an age in which doctrinaire radicals were campaigning for fierce democracy: for the inalienable rights of working men in the country and the “commonalty” in the corporations. Geoffroy’s deistic science had been appropriated by radicals antagonistic to the entrenched power of the Established Church and monopolistic, undemocratic elites in the RCS and ZS. Owen’s academic position in these institutions, and his wider social role as an Anglican anatomist canvassing for patrons among the legal and medical gentlemen of Lincoln’s Inn, made any concession to the extremists unthinkable. The profound ideological differences between Owen and Grant manifested at a professional and societal level, most obviously in Grant’s eviction from the Council of the ZS and Owen’s subsequent consolidation of power. Owen’s awareness of the social consequences of a bestializing Lamarckism made a damning refutation all the more urgent. The question now is to determine the extent to which he utilized the available scientific resources to convince fellows of the GS and ZS, and the wider patrons of respectable science, that Lamarckism was *scientifically* untenable.

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48. Bartholomew, op. cit. (35), 281; Ospovat, op. cit. (44); P. Corsi, "The Importance of French Transformist Ideas for the Second Volume of Lyell's Principles of Geology", *Brit. J. Hist. Sci.*, 11 (1978), 221-4.

49. Corsi, ibid. 241.

50. Bartholomew, op. cit. (35), 292 n.16; Bowler, op. cit. (6), 35, 43 n.64.

51. Bartholomew (pers. comm.) has confirmed to me that his linking of Lyell and Grant was tentative and circumstantial.

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53. Wilson, op. cit. (40), 315-6.

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56. Ibid. i, 257.

57. Ibid. i, 397.

58. Ordinary Minute Book 4, 1828-1830; ff. 14, 371; Book 5, 1830-1832, ff. 74, 85, 119, 273, 229, 331: Geological Society MS.

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133. *Ibid.* 1 (1835-6), 586.

134. *Ibid.* 2 (1835-6), 566, 647, 676.

135. K. F. H. Marx quoted in *British and Foreign Medical Review* 15 (1843), 24.

136. R. E. Grant, *On the Study of Medicine: Being an Introductory Address delivered at the opening of the Medical School of the University of London, October 1st, 1833* (London, Taylor, 1833), 4-5.

137. *Ibid.* 17.

138. *Ibid.* 19.

139. *Medico-Chirurgical Review*, 20 (1833-4), 152-3.

140. *The Lancet*, 1 (1833-4), 73.

141. *Ibid.* 279.

142. *Medical Gazette*, 1 (1828), 1-3.

143. *The Lancet*, 1 (1828-9), 1.

144. C. Bell, *Letters of Sir Charles Bell* (London, Murray, 1870), 299.

145. *Medical Gazette*, 13 (1833-4), 22.

146. *Ibid.* 49-53 (51). On the entrenched opposition of the RCS, RCP, and London hospitals and medical schools to the granting of a charter for the university (a privilege to “annihilate” all existing privileges in the *Gazette*’s view), and the respective petitions to the Privy Council, see H. Hale Bellot, *University College London 1826-1926* (London, University of London Press, 1929), 229-31.

147. G. Webster, “Address [to the British Medical Association]”, *The Lancet*, 1 (1836-7), 593-9 (598).

148. “Dr. Grant and the College of Physicians”, *Medical Gazette*, 13 (1833-4), 119-21 (120). Cf. the rejoinder by “One of the Forty-Nine”, *ibid.* 165-6.

149. Ibid. 292-3.

150. Wakley was beaten and his home fired in 1820 by terrorists (the Spenceans, in sympathy with the Cato Street conspirators), in the mistaken belief that he was the unnamed Argyll Street surgeon who had decapitated Thistlewood and his conspirators after their execution.

151. *Medical Gazette*, 13 (1833-4), 293.

152. Ibid. 7 (1830-1), 372-3; 8 (1831), 218, Davis was proposed a member of the Senate of the projected College of Medicine. See also 9 (1831-2), 21-3 for the *Gazette's* antagonism to Davis, and Bellot, op. cit. (146), 159.

153. On the Crown and Anchor meeting in March 1831 chaired by Joseph Hume to discuss the foundation of a democratic College of Medicine see *The Lancet*, 1 (1830-1), 846-66 (esp. 857). This meeting was well attended, thirteen hundred members of the profession turning up: ibid, 821-3. Actually since the later 1820s, during his impassioned attacks on the corporations, Wakley had been canvassing for such a new national body “founded entirely upon two great principles – EQUALITY OF TITLE, and EQUALITY OF RIGHT”: ibid. 179-82 (182); also 568, where he uses the social metaphor of the “revolutionary” changes of knowledge in order to highlight “aristocratic conceit and blindness” and justify a democratic reorganization in medical government. His editorials against Owen’s RCS also ended with an apocalyptic warning that a new college must sweep away monopolies, again exclaiming that “in our profession a revolution is much wanted”: ibid, 598. On science as an “ideological instrument” at the time of social disturbance during the Regency period see M. Berman, *Social Change and Scientific Organization: The Royal Institution, 1799-1844* (London, Heinemann, 1978), 104-5.

For an antagonistic establishmentarian view of the attempts by Wakley, Hume, and “the humbler and more obscure personages connected with our profession” to get up a “complete and perfect College of their own” see the *Medical Gazette* 7 (1830-1), 792-3 (792), 8 (1831) 21-3. The scheme fell through, probably because the radicals lacked any real hope of gaining a Charter and thus acquiring the power to rival the corporations. Incidentally, by its new charter in 1843, a new group of “Fellows” was instituted at the RCS, and from these the Council (previously a self-appointed body) was forthwith to be *elected*, so the democratic reform movement did make inroads, even if it never went far enough for Wakley.

154. *The Lancet*, 1 (1833-4), 644-5.

155. Ibid. 645.

156. *Medical Gazette*, 13 (1834), 22, 677; 15 (1834-5), 809.

157. Ibid. 13 (1834), 677.

158. *The Lancet*, 1 (1841-2), 163.

159. Ibid. 1 (1836-7), 173, 224, 233, 596. The membership quite consciously envisaged itself unionizing for self ‘protection’: *ibid.* 597, 599, 603. As one delegate said, tradesmen unionize for their collective good, why not GPs (*ibid.* 599), and Webster saw the association’s aim to lobby parliament to enact liberal laws (597). The Association’s first act was to petition parliament to re-examine the system adopted by the Poor Law Commissioners for affording medical relief to the sick-poor: *ibid.* (698).

160. Webster, op. cit. (147). The union stood on similar ground to Wakley when proposing his new College of Medicine, i.e. in support of democratic elections, uniform titles, etc. Again, it all proved too extreme for more cautious delegates at the first Exeter Hall meeting of the proposed Association on 19 January 1837: A. T. Thomson elicited raucous laughter from the radicalized members by imploring that the Association inquire into abuses, not plunge “at once into a sort of radical reform”: *ibid.* 602.

Webster was one of Grant’s “oldest friends” and an Edinburgh contemporary. He acted as a secretary to the fund-raising committee for Grant’s testimonial in 1853. At the presentation he spoke of Grant’s “moral and intellectual worth”: *The Lancet*, 1 (1853), 141.

161. Webster, *ibid.* 595. R. E. Grant, *On the Present State of the Medical Profession in England; being the Annual Oration delivered before the members of the British Medical Association, on the 21st October, 1841* (London, Renshaw, 1841), 70.

162. Grant, *ibid.* 6-7, 25-7, 29-30, 30-2, 41, 54-61, 88-90, 97-8.

163. Ibid. 49-51.

164. R. Owen, “Report on British Fossil Reptiles, Part II”, *Report BAAS*, Plymouth 1841, 60-204 (197).

165. Grant, op. cit. (161), 73.

166. An expression early in use, e.g. E. Forbes to R. Owen, 2 November 1846, BM(NH) OC MS, Vol. 12, f. 308.

167. *Medical Gazette*, 13 (1834), 676.
168. *The Lancet*, 9 (1826), 693; 1 (1824), 305.
169. Sprigge, op. cit. (125), 304; Brook, op. cit. (113), 120.
170. R. J. Godlee, “Thomas Wharton Jones”, *Brit. J. Ophthalmology*, 93 (1921), 154-81.
171. R. Owen, “On the Osteology of the Chimpanzee and Orang Utan”, *TZS*, 1 (1835), 343-79 (343).
172. Desmond, op. cit. (6), Chs. 1 and 2.
173. R. Owen, Hunterian Lectures 1 and 2, May 2 and 4 1837, “Manuscript Notes, and Synopses of Lectures. Owen: 1828-41”, BM(NH) OC 380 f. 81.
174. Desmond, op. cit. (6), 46; J. H. Brooke, “Richard Owen, William Whewell, and the *Vestiges*”, *Brit. J. Hist. Sci.*, 10 (1977), 132-45.
175. R. Owen, MS Notebook 7 (January-May 1832), BM(NH), f. 64.
176. R. Owen to W. Clift, n.d. BL Add. MS 39,955, f. 263.
177. E. Turner to J. Mill, 30 April 1831, reproduced in *The Lancet*, 2 (1835-6), 844.
178. For a critical appraisal of Huxley’s ideological position and relevant references, see Desmond, op. cit. (6), Ch. 1.
179. C. Darwin to T. H. Huxley, 1 January 1860, MS Imperial College, Vol. 5, f. 94: reproduced in Desmond, op. cit. (6), 60.
180. For a rather more optimistic assessment of London lecturing opportunities, see J. N. Hays, “The London Lecturing Empire, 1800-50”, in Inkster & Morrell, op. cit. (3), 91-119.
181. Rev. Owen, op. cit. (75), i, 33, 61, 68; RCS MS n.d. 275(18)h7; RCS MS 275(u)h7. Owen resided with Clift’s family until 1832. The (presumably) part-financial circumstance of being forced into such close proximity to the family led to its own problems, particularly when his love affair with Caroline was discovered: Caroline Clift to her mother, 16 May 1832; BL Add. MS 39,955, f. 218.
182. *The Lancet*, 1 (1836-7), 21.

183. In recommending Owen for the post of House Surgeon to the Birmingham Hospital, Clift characterized him as “sober and sedate very far beyond any young man I ever knew” and affirmed that he would “set all your students a good example for close application, and attention to their professional and moral duties”: W. Clift to J. Hodgson, 7 January 1830, MS BM(NH) OC, Vol. 8, f. 113.

184. UCL College Correspondence Applications MS.

185. Turner, op. cit. (177).

186. Managers’ Minutes (1832-1853) MS, Royal Institution Archives, Vol. 8, ff. 307, 552.

187. Ibid. Vol. 9, f. 15.

188. Managers’ Minutes MS, op. cit. (186).

189. R. Owen to Managers, 29 June 1837, ibid. Vol. 8, f. 307.

190. Ibid. ff. 514, 549, 552, Vol. 9, f. 2.

191. Minutes of Council, Zoological Society MS, Vol. 2, f. 389; Vol. 3, f. 89, 107, 354.

192. [W. J. Broderip], “The Zoological Gardens-Regent’s Park”, *Quarterly Review*, 56 (1836), 309-32 (331).

193. *The Lancet*, 2 (1834-5), 199.

194. Lyell, op. cit. (12), i, 397. Minutes of Council MS, Zoological Society, Vol. 3, f. 17. Also *Reports of the Council and Auditors of the Zoological Society of London, Read at The Annual General Meeting, April 29, 1834* (London, Mills, Jowett, & Mills, 1834).

195. *Zoological Magazine*, No. 2 (1833), 61. Only 6 numbers of Owen’s *Magazine* appeared.

196. R. E. Grant, *Outline of a Course of Lectures on the Structure and Classification of Animals, to be delivered to the Members of the Zoological Society of London, in their Museum, to commence on Tuesday the 15th of January, 1833, and to continue on the succeeding Tuesdays and Thursdays, at Half-Past Seven o’clock P.M.* (London, Mills, Jowett, & Mills, 1833), v, 6-26.

197. Minutes of Council MS, Zoological Society, Vol. 3, f. 290.

198. Grant, op. cit. (196), 18.

199. R. Owen, *Memoir on the Pearly Nautilus* (London, Taylor, 1832), 1.

200. Minutes of Council MS, Zoological Society, Vol. 4, f. 158.

201. *The Lancet*, 1 (1836-7), 766.

202. F. Darwin (ed.), *The Life and Letters of Charles Darwin* (London, Murray, 1887), i, 273-4.

203. Broderip, op. cit. (192), 321; Rev. Owen, op. cit. (75), i, 96.

204. BS 690.

205. E. T. Bennett to R. Owen, n.d.(1835), BM(NH) MS OC, Vol. 3, f. 190.

206. Rev. Owen, op. cit. (75), i, 169.

207. J. Toynbee to R. Owen, 8 June 1841, BL Add. MS 39,954, f. 23.

208. Richard Freeman, University College (pers.comm.).

209. [E. Forbes], “Vestiges of the Natural History of Creation”, *The Lancet*, 2 (1844), 265-6.

210. T. H. Huxley, “On the Reception of the ‘Origin of Species’”, in Darwin, op. cit. (202), ii, 251-2.

211. W. Irvine, *Apes, Angels, and Victorians: Darwin, Huxley, and Evolution* (Cleveland, Meridian, 1959), 38.

212. R. E. Grant to C. C. Atkinson, 19 April 1837, College Correspondence MS 3968, UCL. Hobson was enrolled in Grant’s classes from 1836-8, and was his Gold Medallist in 1837-8.

213. E. Hobson to R. Owen, n.d., RCS MS, Cab. VIII (I)a71.

214. See the lengthy MS correspondence from Grant to Atkinson during April and May 1841, with Owen’s replies, in College Correspondence UCL.

215. On Owen’s acquisitive nature see especially the obituary by his junior colleague at the British Museum H. Woodward, “Sir Richard Owen”, *Geological Magazine*, 10 (1893), 49-54. Also Desmond, op. cit. (6), passim for an extended discussion of psychological factors in palaeontology; and M. Benton, “Progressionism in the 1850s: Lyell, Owen, Mantell and the Elgin fossil reptile *Leptoleuron (Telerpeton)*”, *Archives of Natural History*, 11 (1982), 123-36.

Chapter 6

Owen's Use of Scientific Resources

We have argued that Owen and the medical and legal gentlemen of Lincoln's Inn shared a set of political, social, and moral assumptions; and that, given the contingency of a materialistic Lamarckism mated to Wakley's brand of monopoly-hating radicalism, it was in Owen's social interest to develop substantial anti-transformist ploys. He was, as we shall see, strongly aligned with the Oxbridge dons – particularly the Peelites Whewell and Buckland – whose works epitomized Establishment sensitivity to the threat to Creative interference (1). Sedgwick was not alone in imagining that any tampering with the evidence of the Deity's Personal attention to nature would herald civil strife and social collapse (2). One can understand their obsession with the iniquities of Lamarckism knowing that Continental materialism was fuelling the democratic challenge and underpinning progressive social ideologies, turning working men towards action to achieve 'evolutionary' betterment in this life without waiting for the next. Located in this Tory Anglican context, patronized by Oxbridge-educated physicians and surgeons in Pall Mall and Lincoln's Inn, Owen sustained a theistic interpretation of nature in which man was anatomically modified to become "the seat and instrument of a

rational and responsible soul” (3) – and “responsible” meant socially accountable in this life and morally in the next.

In this chapter we deal with the way Owen’s anti-Lamarckian ideology manifested in the construction and evaluation of esoteric anatomical and palaeontological knowledge. Thus I am proceeding in the reverse order from Steven Shapin, who has argued that an empirical sociology of knowledge must begin by demonstrating “the undetermination of scientific accounts and judgments” and proceed to show *why* particular scientific stances were adopted in specific contexts – i.e. display “the historically contingent connections between knowledge and the concerns of various social groups in their intellectual and social settings” (4). Here I have established a framework of Owen’s social and institutional commitments, and will now demonstrate how his anti-Lamarckian ideology actually became ‘constitutive’ (to use R. M. Young’s word (5)) in his science, and how it guided his selection or rejection of ‘facts’. Thus for working historiographic guidelines I am employing Shapin’s “instrumental model”, in which

the generation and evaluation of knowledge is treated as goal-directed. Knowledge ... is produced and judged to further particular collectively sustained goals. Knowledge, in this perspective, is always tailored to doing things. It is in the course of doing things with knowledge that its meaning is produced; thus, the notions of use and meaning are intertwined (6).

Although Owen’s behaviour can be treated as goal-orientated, this is to be understood in the sense of conforming to

particular cultural values and social interests. We have to beware imputing *conscious* motives, when Owen's scientific actions were the outcome of complex social, institutional, and religious mediations. From the perspective of the Anglican community of King's Counsels and clerical dons his actions would have seemed natural, justifiable, and in perfect accordance with 'dispassionate' inductivist tenets. Only by paying close attention to their *effects* – the destruction of Lamarckian serial ascent with the newly fabricated dinosaur or Geoffroy's unity of composition using von Baer's embryology – can one really judge the social worth of Owen's science in a specified context. We find that, e.g., his dinosaur was crucial in demolishing transformist tenets, which made it useless to middle-class mercantilist Darwinians a generation later, forcing them to scrap the beast and build it afresh to evolutionary specifications.

To illustrate the way available scientific resources were refashioned for anti-Lamarckian ends, I shall instance three episodes between 1832 and 1841 in which Owen devised a culturally *useful* response (i.e. meaningful to the broader Anglican elite). The three follow in sequence, and show Owen identifying with increasing accuracy the weak points of the transformist target. (Indeed, in the opening debate with Geoffroy, over *Ornithorhynchus*, transformism was not mentioned by name, even though the outcome would materially affect the transformist argument.) His notebooks and papers suggest that the three serious areas of concern were: 1) the

transitional nature of monotremes (Geoffroy's word), Owen attempting to refute the alleged evidence of their oviparity in 1832-4; 2) the ontogenetic development of the chimpanzee, his reinvestigation in 1835 being used to distance the ape from man; and 3) the palaeontological argument for progressive development, Owen elaborating a retrogressionist strategy, which culminated in his origination of the 'dinosaur' in 1841.

One final word about his identification of the 'target'. More often than not he singled out French materialists (Bory Saint-Vincent) or transformists (Geoffroy and Lamarck), often only citing Grant in parentheses or notes. There was good reason for this, and it by no means invalidates the thesis. First, the theoretical content of Grant's lecture course was mostly French, thus Owen was attacking it at source. By this means, he could (or thought he could) avoid an unseemly personal confrontation with Grant and maintain a gentlemanly decorum. But more importantly, as a nationalistic gesture denouncing French atheisms and materialisms and their "train of monstrous consequences" (7), he gained immense capital as a guardian of social stability in Britain. In other words, the stratagem had a career pay-off; it was the kind of nationalistic noise that appealed to a broad spectrum from Coleridgean "patriots" like Abernethy to conservative Whig dons like Sedgwick.

It was also easy to single out Geoffroy. Among contemporaries he so often evoked a mixture of deference and despair. When, at the height of his “Paper War” (8) with Owen in 1833, Geoffroy wrote to Clift asking for information of *Ornithorhynchus*, the Irishman Joseph Pentland – who actually had to work alongside Geoffroy at the Muséum – put in a covering note calling him a “terrible *wrong head*”, although conceding that his position as Professor at the Garden & president of the Royal Academy of Science for the present year merit some considerations” (9). Owen had seen Geoffroy at the Institute; Grant might even have introduced them in 1831. But I have seen no evidence that their acquaintance was anything more than passing. As if in confirmation, the Owen Correspondence at BM(NH) contains no letters addressed by Geoffroy to Owen, even though Geoffroy’s letters to E. T. Bennett and William Clift (10) – at least where they concern *Ornithorhynchus* – were passed on to Owen. Hence Owen was more personally detached and could be critical in print (whereas he was still friends with Grant, whose position at the ZS during the *Ornithorhynchus* debate might have made confrontation for the young Owen professionally dangerous). Not that Owen was wholly unsympathetic to Geoffroy’s morphological science; indeed, he accepted a limited “unity of composition” (see next chapter), and even believed that Cuvier had underrated the theory of analogies. However he insisted that Geoffroy and his followers had pushed the principle too far, and that Geoffroy’s principle of correlations did not justify his opercular-ear ossicle analogy.

But

it was precisely because Owen did accept a limited ‘unity’, and because rivals like Grant saw transformist ramifications in the extended doctrine, that it became essential for Owen to disavow the alleged mutationist implications.

In this chapter then we will concentrate on his threefold refutation of transformism before proceeding to his restructuring of theoretical issues in the next.

1) The Generation of Monotremes

In the 1820s many anatomists classified monotremes as mammals (usually edentates) while believing with John Hunter’s brother-in-law Sir Everard Home (1756-1832) that the rare *Ornithorhynchus* (platypus) was in some sense transitional. Home wrote in the *Philosophical Transactions* in 1819 that the ova of the kangaroo, wombat, and platypus constitute rungs in the “chain of gradation” (11). He considered that egg formation in the platypus placed it midway between “the American opossum and the bird”. Because, he thought, the young platypus hatched in the oviduct (ovoviparity), after which it provided for itself, the mother “not giving suck”, he was able to construct a “beautiful series”: marsupials connect quadrupeds with the “ornithorhynchi”, and “these again approach so nearly to the bird, as to complete the links of gradation between the human species and the feathered race, so far at least, as concerns

their mode of generation". But it was noticeably the transformists who *separated* the monotremes into a distinct class. Lamarck in *Philosophie Zoologique* had speculated that they were not mammals, "for they are without mammae, and are most likely oviparous" (12). Nor were they birds or reptiles; so he created a distinct class, describing them as "*Animals intermediate between birds and mammals*" (13). But this was no longer tenable by the 1820s, a number of anatomists having found only superficial similarities to the bird. Robert Knox, dissecting a male platypus shipped by the Governor of New South Wales, Sir Thomas Brisbane, to Jameson's Museum, concluded that the generative organs were unlike a bird's, the nervous system was mammalian, and

the same observation is applicable to the bones and muscles, so that the analogy supposed to exist between the Ornithorhynchus [sic] and Birds is reduced to the resemblance of the ossicula of the ear, and to the female organs of generation ... (14).

Lamarck's suggestion of oviparity was however supported by Geoffroy, who placed monotremes between *reptiles* and mammals in a fifth vertebrate class. If anything, he believed them to be closer to reptiles. Grant recalled that "The day I left Paris, in September last [1829], that venerable anatomist mentioned to me, that he was perfectly convinced that the ornithorhynchus is a true *oviparous reptile*, from an examination of its structure, and particularly from its organs of generation" (15). However in print in 1830 Geoffroy contended that warm blood and an advanced respiratory apparatus made it "nécessaire de voir en eux l'essence d'un

nouveaux type, d'établir pour eux une *cinquième classe* parmi les animaux vertébrés”

(16). He justified this on the grounds of a common cloacal opening, oviparity, and lack of mammary glands. But by 1830 the last two points had become highly contentious. On the question of oviparity, Geoffroy had seized on the description supplied by Grant of two eggs reputedly from an *Ornithorhynchus* nest. Geoffroy had actually printed Grant's letter in his paper announcing the new class, although his need to fortify his position had evidently caused him to read too much into it. Writing to Jameson, Grant showed greater caution; he admitted having examined shells of two eggs in Leadbeater's collection, and despite Yarrell considering them unlike bird or reptile eggs, Grant saw that these cylindrical eggs did “closely, resemble in form and size those which I have found in many Saurian and Ophidian reptiles” (17). But the major problem, as Geoffroy came to appreciate, was that the eggs were actually larger than the *Ornithorhynchus* pelvis, forcing him into some dubious *ad hoc* reasoning.

On the other hand, field reports from Corresponding Members of the ZS in New South Wales did tend to support Geoffroy. With the increase in naval and military activity in the colony at this time, accurate reports were at last reaching London. In November 1831 the Committee received a new platypus, *O. brevirostris*, collected by Lieut. Friend in the Swan River region (18). The following year it received

the first detailed reports of the platypus' lifestyle, from Lieut. Maule of the New South Wales Garrison. He had actually set out to test the colonists' belief "that the female *Platypus* lays eggs and suckles its young". He confirmed that they lived in burrows on river banks. He had managed to dig out several nests and picked up what looked like egg-shell in the debris; and in females that had been shot "eggs were found of the size of a large musket-ball and downwards, imperfectly formed however, i.e. without the hard outer shell, which prevented their preservation" (19). From another nest he took a female with two young; and when she died and was skinned "it was observed that milk oozed through the fur on the stomach, although no teats were visible on the most minute inspection; but on proceeding with the operation two teats or canals were discovered, both of which contained milk" (20).

Such was the available evidence in 1832 when Owen first engaged Geoffroy, and we will see how each selectively sifted the evidence, emphasizing some aspects and repressing others. Take the question of egg-laying first. Geoffroy was prepared to defend this, even if it meant proposing an anomalous reproductive strategy – e.g. allowing the huge egg to develop rapidly in the *cloaca* before being laid. In 1833 he switched to suggesting that the egg might actually have to hatch in the oviduct because it could not pass the restricted pelvis (21). Owen by contrast was convinced that the platypus and echidna were mammals, extraordinary maybe, but mammals

nonetheless; so he was unreceptive to any hint of oviparity or incubation, and endorsed only those facts in Maule's letter which suited him. In his first paper, communicated to the RS by Green, Owen *unreservedly* accepted the observation of milk secretion but was unresponsive to the sighting of egg-shells, suggesting that these observations were of little value because Maule had failed to state the time of year or position of eggs (whether in the ovary, oviduct, or cloaca). And egg-shells in the nest proved nothing because these could have been expelled covered in excretory salts after an ovoviviparous birth (22). In the same way, studying the nestlings shipped back to the ZS by Maule, Owen emphasized only those structures which vindicated him. Hence he enthusiastically noted the presence of "coagulated milk" in the stomach (23), which proved the correctness of Maule's sighting of milk in the mother's fur. Yet he refused to concede that the allantois (which he pointed out was present in embryo birds and ovoviviparous reptiles) suggested oviparity or a yolk-store; nor, more crucially, would he admit the importance of traces of an *egg-tooth* which he himself detected on the bill of the smaller specimen. As he argued in a rather tortuous fashion:

It ... is obviously analogous to the horny knob which is observed on the upper mandible in the foetus of some *Birds*. I do not, however, conceive that this structure is necessarily indicative of the mandible's having been applied, under the same circumstances, to overcome a resistance of precisely the same kind as that for which it is designed in the young *Birds* which possess it. The shell-breaking knob is found in only part of the class [note the

spurious logic: the fact that some birds lack it says nothing about its shell-breaking function in those which possess it!]; and although the similar caruncle [egg-tooth] in the *Ornithorhynchus* affords a curious additional affinity to the Aves, yet as all the known history of the ovum points strongly to its ovoviviparous development, the balance of evidence is still in favour of this theory (24).

So the *value* Owen set on Maule's observations varied according to whether or not they corroborated his prior theoretical position. Between 1832 and the time of his second paper to the RS in 1834, Owen formulated a powerful set of interlocking anatomical and physiological arguments, designed to *disprove* Maule's inference of oviparity and incubation in favour of ovoviviparity. These were 1) the lack of detectable shell-secreting membranes in the uterus, 2) the narrowness of the pelvis and lack of avian-like modifications, preventing a large egg being laid, 3) the lack of sufficient yolk in the ovum to enable the embryo to survive through incubation, and 4) the presence of mammary glands, which in mammals substitute for yolk in the bird and render incubation superfluous (25). By this means Owen attempted to build up a watertight *logical* case, founded on anatomical evidence of preserved specimens, to discredit aspects of Maule's *observational* evidence. Owen ended by presenting a veritable *reductio ad absurdum*: "If, therefore, the gestation of the *Ornithorhynchus* terminates by the exclusion of an egg, as in the Bird or Tortoise, the preparatory steps in the formation of the ovum are widely different, for the parts concerned manifest the essential characters of the mammiferous type, and the germ itself has a corresponding structure" (26).

Rounding off, Owen drew spurious positive conclusions from negative evidence. He now offered his own opposing piece of *observational* evidence. This was the testimony of George Bennett (1804-1893), who had been dispatched to the colony to gather just such material. He reported back on his discovery of a nest with three naked young a little over an inch long, presumably newly born. “The nest was most carefully scrutinized by Mr. BENNETT”, wrote Owen, “but not the slightest trace of egg-shell could be perceived in it” (27). But such negative evidence was of dubious value and did not carry the same weight as Maule’s positive sighting. Finding no eggs of course does not mean that no eggs were laid.

Owen’s attempt to circumvent the counter-evidence did not convince everyone. John Marshall of the Military Museum in Chatham considered Owen’s “Paper War” with Geoffroy a perilous affair. His museum possessed a platypus preserved in spirit with four eggs in the oviduct, and he invited Owen to view it (28). Geoffroy quickly learned (“I know not from whom”, puzzled Pentland (29)) of this further proof of a gravid female, and quizzed Clift for details (30). Owen visited the Museum at Fort Pitt to examine the specimen (31), discovering three well developed ovisacs in the ovary which he considered perfectly in accord with ovoviparous development, telling Geoffroy that the ovules resembled “a tous egards” those he had already figured, and that no eggs were found in the oviduct or uterus (32). They offered, in

Owen's view, no evidence to subvert his ovoviviparous theory.

In the foregoing, I have deliberately emphasized Owen's logical irregularities and strategic devaluation of conflicting evidence; over-emphasized it, since I wanted to highlight the consequences of Owen's unshakable conviction in the mammalian nature of monotremes, and his elevation of this belief beyond the reach of empirical refutation before moving on to the anti-Lamarckian value of that conviction. If I have seemed to do Owen an injustice I will correct it. I am implying nothing disingenuous on his part; quite the reverse, since selection is part and parcel of normal evaluative practice in science. Owen was not consciously distorting, even less did he imagine himself having an ideological axe to grind. This would have involved a moral breach, if nothing else, since the discovery of inductive truth and the eternal verities was a statement about Divine creation and as such a theological imperative*. We must believe Owen when he declared himself "in no way biassed" by his belief in "the mammiferous nature of the Ornithorhynchus" (34). Ironically,

*On the other hand, Owen was one of the few anatomists honest enough to admit publicly that ethical considerations could dictate the *content* of one's science – as when he inclined towards the reflex concept and implied automaton theory of lower life, on the grounds that it was incompatible with Divine beneficence that, say, *conscious* polyps should undergo the indescribable agony of being grazed alive by predatory fish (33). Knowing this, it is *possible* that he brazenly avoided Geoffroy's deductions for fear of underpinning his transformist speculations, and thus bestializing man, which would have been equally incompatible with Divine intent. While this might have been a deep underlying factor, I doubt that Owen formulated it consciously in 1832.

about the only thing one can accuse him of is imputing apriorist motives to Geoffroy (35).

We have gained an unfair impression of the weakness of Owen's position by concentrating on anomalies like eggs and incubation which were crucial to Geoffroy's case. Owen's real strength lay in his elegant demonstration of the existence of mammary glands in monotremes. On this subject he encountered Geoffroy already on the defensive, and he was now able to push him into defending an increasingly untenable position. As early as 1824 Meckel had detected tiny glands composed of tubular tissue, which he interpreted as mammary. Meckel believed that monotremes approached closer to oviparous animals than marsupials, but nonetheless agreed with Cuvier that they *were* mammals (he placed them next to edentates). Geoffroy too detected this gland in monotremes but reported that it possessed none of the characteristics of marsupial mammary glands. The tissue structure was different, nipples were lacking, and the gland was smaller than Meckel suggested (36). (It did not occur to him that this size difference represented a sexual or seasonal variation; Owen was far more astute on the possibility of experimental investigation of this question.) Geoffroy finished by proffering two alternative hypotheses: either these were lubricating glands analogous to those on the flanks of salamanders (an aquatic adaptation), or corresponded to the scent glands of shrews, glands which follow the phases of sexual development, and whose odour conveys the female's sexual state.

Owen's papers were designed to sustain Meckel's interpretation; and since Owen invariably prefaced his papers with a statement that the outcome of the debate over the gland would settle the "true affinities of the Monotremata" (37), he was establishing premises which were destined to support broader conclusions. Owen proved the milk-secreting function of these glands and discredited Geoffroy's rival speculations by an ingenious comparative study of five adult females. His procedure was to measure the size of the gland in each (they ranged from one and a half to over five inches in length, so the degree of development was easy to assess). He then dissected out the uterine organs and gauged the degree of ovarian development in each individual. This enabled him to relate gland-size to the ovarian cycle. He recognized three stages: 1) when the glands were smallest, so were the ovaries, indicating a cessation of function in both; 2) the gland had slightly enlarged when the left ovary reached its maximum of development, and contained eggs the size of small peas; and 3) when the glands had arrived at *their* full size, the ovaries were declining, having released the eggs. In other words, "the period when these glands exhibit the greatest activity, appears to be *after* gestation" (38), as one would expect of a lacteal gland. The clever point was Owen's simultaneous elimination of Geoffroy's counter-proposal. In the second stage, the gland had only just begun developing when the eggs were full-size. Had the

function of these disputed glands

been to secrete, as Professor GEOFFROY supposes, an odoriferous substance attractive of the males their maximum of development ought to have been exhibited in this specimen, in which the uteri evinced, by their size and vascularity, traces of high excitement, and the ova appeared ripe for impregnation. The greatest development of the abdominal glands, on the contrary, was observed where the ovary appeared to have recently executed its function (39).

Owen's experimental strategy was perfectly executed. He displayed a mastery of difficult dissection techniques (perfected through his examination of deceased animals in the Zoological Gardens), allied to a clear conception of the case to be proved. Owen could then call up Maule's corroborative observational evidence of a lactating female and young with milk in its stomach. And he clinched his case by dissecting out glands of similar structure in the *Echidna*, demonstrating its affinity to the platypus, despite appearances. He could now assert that the monotremata as a group accord "with the rest of the *Mammalia* in the characteristic function of lactation" (40).

Correspondence between Geoffroy and the ZS increased dramatically between February 1833 and April 1834. Geoffroy's constant position shifts testified to his difficulty, and his retreats were partially disguised by effusive outbursts of special pleading. In February 1833 he resorted to a structure/function argument. In a letter to the ZS, he listed the distinguishing features of the platypus' urino-genital system, concluding that its organization "is that of a

Reptile; now, such as the organ is, such must be its function; the sexual apparatus of an oviparous animal can produce nothing but an egg" (41). He now speculated with no evidence that the abdominal gland might in fact secrete a lime compound to harden the shell – an additional anomaly allowing him to plead for “further examination” rather than rushing to accept the beast’s “normality, founded on strained and mistaken relations, which invites indolence to believe and slumber”. This sort of tactless communication did more harm than good, and Owen gave his baseless speculations short-shrift in a follow-up note read after Geoffroy’s letter. By March Geoffroy had assimilated Maule’s observations and made a new effort to prove that the fluid secreted was not milk but mucus, on analogy with that from the odoriferous glands surrounding the mammary glands in shrews – pointing out that these too enlarge during sexual excitement. In the case of monotremes Geoffroy guessed that the mucus thickened by water might serve to nourish the young. And he ended by again pleading against taking a “retrograde” step and enforcing an unnatural union when the facts demand we place monotremes “further within the limits of oviparous animals” (42). But again Owen countered Geoffroy point by point, dismissing his dubious structure/function argument (that “conglomerate” mammary glands produce milk, therefore the simple caeca in monotremes must have another function) by pointing out that whales too possess simple caeca. Nor could mucus thickened in water have nourished the

young *terrestrial* echidna.

The “Paper War” ran on through July, with Geoffroy surreptitiously branding Owen a reactionary (being hidebound by “the rules of the *past*”) while placing himself on the side of “*Progress*”. Again he shifted ground, this time to make it a gland *sui generis*, which achieved its maximum development in monotremata – and it was this “*Monotrematic*” gland which now justified the taxonomic distinction. Geoffroy was being forced to take increasingly precarious steps, and his *ad hoc* theorizing reached a climax when, in answer to Owen and von Baer, who had both raised the case of whales, he suggested “not that the *Monotremata* should be thrown back into the centre of the *Mammalia*, but that the *Cetacea* should be separated from among them” (43).

This blow-by-blow account provides one of the most graphic examples of piecemeal scientific retreat, desperate *ad hoc* hypothesizing, and recourse to special pleading when scientific manoeuvres failed. The fact that the parries and ripostes were appearing sometimes bimonthly allows us a unique insight into the detailed mechanics of refutation and shows the tenacity with which a theory will be held when it directly supports one’s philosophical view of nature, itself resting on inviolate social or ideological foundations. Geoffroy forfeited his initial support by his lack of subtlety, and he was forced to concede early in 1834 that the “monotrematic” secretion in porpoises really was

milk (44). Owen's success reflects his astute assessment of the form which a convincing refutation must take. He often began his papers by suggesting that Geoffroy's new class must stand or fall with the outcome of studies on this gland; thus he introduced into the initial equation the elements that would allow him to draw definite anti-Geoffroyan deductions. Thus, before describing his *Echidna* preparation he told the ZS fellows:

It is well known that the idea of constituting a new class for the separation of the *Monotrematous Quadrupeds* of New Holland, and of separating them altogether from the *Mammalia*, arose chiefly from the supposition of the total absence of a mammary apparatus in them. This circumstance was at the same time regarded as a strong proof of an essential difference in their mode of producing the young: and it was inferred that the latter, in the absence of the lacteal nourishment, must have derived the materials necessary for their development from some store of nutriment analogous to the yolk of the embryo in the oviparous and ooviviparous tribes (45)

From the proof that the gland was mammary it must follow that the mode of generation was normal. As the absence of milk once forced naturalists to infer a yolk-filled egg, so the discovery of lactation made this unnecessary. Owen's case was strong and convinced potentially hostile critics on both sides of the Channel. Those with convergent interests now deserted Geoffroy. Blainville (whose animal chain might have benefited from an intermediate class) came out on Owen's side (46). Grant, lecturing on monotremata at LU in March 1834, treated the debate like distant history. Or rather, he failed to mention the *debate*, or for that matter Owen, but

co-opted Geoffroy for the ‘winning’ side:

With a divided uterus like oviparous animals, it was imagined that these edentulous genera of New Holland must lay eggs like a bird or a reptile, till it was observed, that as we descend through the inferior mammalian as the ruminantia, the uterus becomes more and more completely divided, and at length, in the lower rodentia, as in the rabbit, it is cleft throughout its whole extent, as in the *monotrema* and *marsupialia*, without entirely altering their viviparous mode of generation. And from having no nipples, it was supposed that the *monotrema* had no mammary glands, till the investigations of MECKEL and GEOFFROY SAINT-HILAIRE demonstrated the existence and the structure, and all the relations of these organs, and several observers in New Holland showed that they secreted milk (47).

Grant’s lecture showed his close acquaintance with monotreme anatomy. He urged that their primitive traits be “viewed as marks of inferiority generally”, rather than suggesting a special affinity with one or other of the reptilian or avian classes. He admitted their resemblance to edentates, but thought the “low condition of their generative system conveniently separates them as a separate order” (48). So Owen – acknowledged or not – had successfully convinced at least the London Geoffroyan of the monotreme’s mammalian nature; and after 1834 the platypus was conventionally placed in a discrete order below the Edentata in the Mammalia.

Owen had achieved his main aim: to discredit the alleged oviparity of monotremes and thus throw doubt on their *intermediate* nature. That this was construed as an anti-Geoffroyan stand by the London elite there can be no doubt. In May 1834

Owen was elected FRS with support from both Lincoln's Inn surgeons (Green, Brodie, Blizzard, etc.) and the ZS Tories (Broderip, Sabine, Kirby, etc.) (49). Another factor contributed largely to his success, besides his logical and zoological acumen: this was the superiority and exclusivity of his material, reflecting first the wealthy RCS's ability to invest its funds in ideologically-appropriate ways, but also related to this it reflects Britain's colonial and military expansion in New South Wales, and the establishment of garrisons on the Swan and Fish Rivers. One reason that Owen could cut so quickly through the Meckel-Geoffroy stalemate was that he could muster five adult female platypuses for comparison. He was in the enviable position of actually being able to request the anatomical parts he needed from George Bennett. Bennett had become a member of the RCS in 1828, at which time he met Owen (50). After returning from a first trip to Australia in 1831, he was present when Owen dissected *Ornithorhynchus* in 1832 (51). So on returning to the colony that year, Bennett carried equipment supplied by the College and Owen's list of required specimens (52). Filled crates were shipped home in the summer of 1833, and by May 1834 the total number of specimens received exceeded five hundred. Many were unique and in a "good state of preservation", and accompanied by "copious and accurate observations on the locality, temperature, time of day, and the circumstance connected with the capture of each specimen" (53). Of overriding importance were "3 unique specimens" of impregnated female organs with small ova, which became the

subject of Owen's 1834 paper, as well as male and female generative organs, glands, and so on. Bennett was duly awarded the College's Gold Medal in recognition of the value of these shipments. Being able to put an assistant so quickly in the field gave Owen a distinct advantage; by July 1833 he was exhibiting Bennett's specimens at the ZS, and reading extracts of his letters into the *Proceedings* ("...the milk gland is very large; and I can now inform you from actual observation that milk is secreted from it ... you can mention it to the Zoological Society as a decided fact ..." (54)).

This colonial connection of course left Geoffroy at a disadvantage, and he was continually forced to request information from Grant, Clift, or the Secretary of the ZS. His requests for drawings and transcripts of the colonists' letters were always courteously complied with; but, with the exception of one of Maule's nestlings being sent to him (55), the material for dissection remained the property of the RCS or ZS. Hence Geoffroy's difficulty in answering Owen with anything like enough detail, and his recourse to speculation and special pleading.

The question of transformism was never raised during the debates although Owen was dealing with known transformists, and Geoffroy's new class had an obvious bearing on the taxonomic gradualism so essential for contemporary transformist theories. Indeed, the tenacity with which Geoffroy clung to his new class – and his unrealistic *ad hoc*

explanations of how a large egg could pass through a small pelvis, or why the abdominal glands must be anything but mammary – strongly suggests that there was more at stake than pedantic taxonomies or structure/function correlations.

2) The Proximity of Ape to Man

Only when faced with the spectre of the transmuted human did Owen bring the subject out into the open. Like Lyell, he was acutely aware of the moral danger inherent of brutalization, and the consequent need for taxonomic detachment from the bestial apes. This became perhaps Owen's overriding concern from the mid-1830s. Lyell was aware in 1826 that the progressionist edifice made man the end product of the sequence; fossil life formed “one connected plan”, of which man was an “inseparable” part (56) – and that he stood in close proximity, osteologically speaking, to the quadrupeds. Bartholomew contends that when Lyell awoke to the awful consequence were Lamarckism to be “ingrafted” (57) onto the scheme he shifted to a safer non-progressionist palaeontology. Owen however took a quite different course. He never doubted that there had been a general progression, even if “not by an uninterrupted succession of approximating steps” (58). He chose instead to challenge the anatomical basis of the premise that man and ape were osteologically close.

With Corsi, I believe that the threat was far more immediate than is realized. It might have been a quarter of a

century since Lamarck speculated on the transition of a quadrumanous animal into man. But we should remember that the *Philosophie Zoologique* was reissued in 1830, when Geoffroy reopened the whole debate. Lamarck's famous "reflections" retained a frightful force. He had implied that if man and ape differed only in organization, then we can construct a sequence of behavioural shifts by which the Angolan orang (chimpanzee) might have acquired human habits. Being forced to the ground, it would have lost its opposable thumb as it habituated to walking; once there it could command a distant view by standing erect, and doing so for generations would result in calf development:

if these same individuals were to give up using their jaws as weapons for biting, tearing or grasping, or as nippers for cutting grass and feeding on it, and if they were to use them only for mastication; there is again no doubt that their facial angle would become larger, that their snout would shorten more and more, and that finally it would be entirely effaced so that their incisor teeth became vertical (59).

Owen's researches effectively turned this from a behavioural possibility to an anatomical improbability. We know it was no straw man he was setting up: Lamarck's behavioural sequence had been strengthened as recently as 1827 by Jean Baptiste Bory Saint Vincent (1780-1846). Corsi has already suggested that we look closely at Bory,. He might not have endorsed Lamarck's universal tendency towards higher organization, but he admired Lamarck's philosophy and systematics (60). Bory was a materialist and treated thought

as a necessary product of organization. He surpassed Lamarck on the orang question. For example, where Lamarck considered the ape considerably inferior to man in intelligence (61), Bory in the *Dictionnaire Classique* supported Tiedemann's conclusion that the brains of the red and black orangs closely resembled a human being's. And he argued that ape brains differ from those of monkeys on just those points in which they approach more closely to man (62). The orang's high organization explained its cultural adaptability. Opponents like Frédéric Cuvier had never denied that adaptability – indeed Cuvier himself had pointed out that the orang understands commands. Why then, asked Bory, does Cuvier deny them reason (63)? Words were not proof of superior reasoning power since “idiots” often spoke distinctly. Bory concluded that the sole feature distinguishing man from ape was speech: orangs lack a thyroidean pouch and cannot articulate words – and Bory hazarded that given such a voice box a chimpanzee might even appear superior to a Hottentot (64).

While Lamarck airily speculated that novel habits might transform apes, Bory instanced the case of resin-gathering peasants of the Marensin canton, who habitually climbed trees for a living and had acquired remarkably dextrous toes. They could write with them – yet inflexibility and a “parallel” big toe were supposed to distinguish man from ape. Shouldn’t the *résiniers*, he asked facetiously, therefore be separated from Bimana and placed with the monkeys? Bory’s shafts might have been aimed at the younger Cuvier, but they struck Owen.

The goading probably convinced him of the fundamental irresponsibility of French materialists. He could not have taken lightly Bory's taunt that if man's big toe was as opposable as the ape's the two species would be zoologically indistinguishable. (Bory refused to take into account the "immortal soul" since it lacked any "anatomical characteristic" (65).) According to Bory it was vanity that drove us to ally orangs with monkeys, and put these among the "stupid brutes", while elevating ourselves to a dignified position. Just this kind of flippancy of course proved immensely upsetting to those like Lyell and Owen at home. However anatomically imponderable the immortal soul, this Divine gift was responsible for man's reason and moral dignity. Bestializing man, convincing him that he lacked a soul, would be to unleash the forces of moral decay and social degeneration.

Evidence for the smooth transition from monkey to man was found in the facial angle. Geoffroy in 1812 had recognized three genera of orangs: *Troglodytes* (chimpanzee), *Pithecius* (orang-utan and gibbon), and *Pongo* ("Wurmb's Ape"). The latter was named after a large ape killed in Borneo about 1780 by a Dutch merchant and described by Baron von Wurmb of the Dutch East India Company, (66). It seemed a more brutal ape with huge teeth, protruding muzzle and smaller brain; thus it was classified below the *Pithecius*, with its more human-like physiognomy. In Geoffroy's "Tableau des Quadrumanes" apes were arranged sequentially and showed

a

gradual approximation to man: Wurmb's ape with its baboon like jaws had a facial angle of 30°, in the flatter-faced *Troglodytes* it was 50° (67). Although by the later 1820s both Bory and Georges Cuvier considered that these were merely age variants (Bory in 1827 cut through the terminological confusion and recognized only "black" and "red" orangs, still the graduated series *Pithecius-Troglodytes-Homo* held because chimpanzees were still only known from immature, flatter-faced specimens. Hence Latreille in *Familles Naturelles du Règne Animal* (1825) conventionally listed apes in terms of decreasing facial angle (68), while Bory mischievously noted that the angle in chimpanzees was barely three degrees less than in Hottentots.

Lyell and Owen knew that Lamarck had sought to explain this muzzle shortening by means of changing dietary habits. But they tackled this heretical explanation of the facial sequence in quite different ways. Lyell admitted that in some circles it was believed that "certain simiae differed as little from the more savage races of men, as do these from the human race in general; and that a scale might be traced from 'apes with foreheads villainous low,' to the African variety of the human species, and from that to the European" (69). While he was willing to concede that a capacious forehead in higher races of *men* might indicate "a large development of the intellectual faculties", he dismissed any parallel "graduated scale of intelligence" in animals as "visionary speculation". As befits the son of the Kinnordy

gentry, he concluded that the ape's "sagacity" had been exaggerated "at the expense of the dog".

In his first paper on the orang read to the ZS before he left for France, Owen had already noted the difference between the adult and baby orang in cranial development but had failed to place this in an anti-transmutationist context. His use of the material was to have changed dramatically by the time of his theoretical *tour de force* "On the Osteology of the Chimpanzee and Orang Utan" in 1835 (70). In 1830-1 Owen was still an unknown entity; his paper was empirical and unprovocative, the sort to impress senior zoologists with his descriptive powers. The subject was a young male orang which had died three days after arriving in Bruton Street from Calcutta (71). Owen investigated the larynx and brain (which externally resembled a man's); and agreed with "those who maintain that [the orang and Wurmb's ape] constitute really but one species, of which the *Orang* is the young The remarkable differences in the crest of the *cranium*, and in the facial angle, appear to be the result of the action of the powerful muscles of manduction..." (72). He was content with no more than neutral 'observation'. Likewise in May 1831 he finished his paper with a comparison of ape and human limb musculature, conventionally relating the "high degree of deviation from the human structure" to the climber's need for flexibility, but drawing no higher, theoretical conclusions.

Four years later when the ZS accepted his new paper (May 1835), the material had acquired a new meaning. By then much had happened: Owen had travelled to Paris, debated Geoffroy until the latter came to “lay down his arms” (73), seen Grant publish his transmutatory lectures, and contributed to his downfall at the ZS. Nor was Owen any longer the novice: institutionally secure, patronized by Coleridgeans, newly elected FRS, about to take the helm at the ZS – there is no doubting his strong social position. Now every aspect of ape anatomy was studied for its anti-transformist meaning. A new theoretical paper couched in anti-Lamarckian language could have a higher nationalistic appeal, as a repudiation of the despised materialist philosophy lying at root of France’s continuing instability. Equally it could serve Owen well on a professional level, increasing his control of ZS resources under the patronage of the Tory Council.

Owen concentrated on the facial angle which he claimed had lent undue support to “theories of animal development” (74). He attempted to use his knowledge of allometric variation to knock the middle rung out of the orang-chimpanzee-human sequence: in short, to show that the hitherto-undescribed *mature* chimpanzee also had a bestial physiognomy and was as distant from man as was Wurmb’s ape. He implied that chimpanzees had been misunderstood because only the unrepresentative young had been studied. He began by devising strategies in his notebook, working out the best way to present his case. He drew a human parallel to prove that the

baby ape's anthropoid cranium was misleading:

The human cranium prior to the development of the deciduous teeth presents a disproportionate preponderance of the intellectual over the animal part, in consequence of the larger size of the brain and the smallness of the masticating organs. At that period, therefore, the skull would afford a very erroneous notion of the endowments of Man as they are characterized by the condition of the bony receptacle of the brain & organs of sense. Much greater is the difference which the skull of the young presents in comparison to adult in consequence of the greater difference between the [2 & pt] sets of teeth.

If a Statuary or Phrenologist were presented with a human skull having the proportions of that of a child before the shedding of the temporary teeth; the one would recognize in it the exaggerated front & facial angle of a demigod and the other predict from its undue cerebral development the intellectual powers of an Aristotle or a Bacon (75).

Chimpanzee endowments had been exaggerated for the same reason. Knowledge of the bigger-brained young had misled classifiers into imagining a taxonomic continuity. In the ZS's *Transactions* he developed this argument. He lamented the fact that the platypus was better known than the species standing next to man "in the natural series" (76). Apparently no adult chimpanzees had been transmitted alive to Europe, and the public museums of Paris and London possessed only skeletons of immature specimens. So on finding an adult skeleton in the private museum of a Curzon Street surgeon, Owen was able to give the first description of the adult form. His declared aims were modest: "to trace in [the chimp and orang] the changes which the skeleton undergoes in its progress towards the mature state; and ... to show the nature and extent of the osteological differences which divide the

Orangs from the human species". But he had in mind considerably more, for that difference was to increase our appreciation of the "modifications" by which a "material organism" is adapted to house a rational soul – it was to support conclusions of a spiritual difference derived from a higher source. More immediately he was striking at the transformists' continuum; and in his preamble he announced his major conclusion:

as it has uniformly happened that the *Orangs* which have been described have been of uniform age, many circumstances, as the facial angle, the forms and proportions of the teeth, and the shape and relative size of the *cranium* to the face, have had an undue importance assigned to them, and the transition from the *Monkey* to the *Man* has been assumed to be much more gradual than a more extended investigation will be found to sustain (77).

Owen detailed the anatomical and morphological differences between adult and juvenile ape. The adult's skull was significantly less human-like; it had broad expansive crests accommodating powerful chewing muscles; the face was correspondingly more prognathous, with protruding jaws and pronounced teeth. The "irrational ape" possessed trenchant canines as "weapons of destruction", something unnecessary for "the master of the animal creation". Owen was painting an essentially bestial picture, giving some idea of the taxonomic vastness separating human and ape architecture.

True, the resemblance of human to infant ape was "startlingly close" (78). But to obviate this difficulty, he

demonstrated how the young ape threw off its human mask to reveal its true nature. Shedding the milk teeth, the jaws elongate, canines protrude, and biting muscles anchored to the massive brow ridges give the jaws a frightening force. Brain growth is relatively retarded, since the “cranial box” remains almost static in size as the jaw apparatus expands. Allometric changes result in the adult dwarfing its brain, explaining why “the docility and gentle manners of the young Ape rapidly give way to an unreachable obstinacy and untameable ferocity in the adult”. So great was this allometric change (he was later to call it a “metamorphosis” (79)) that anatomists might be forgiven for imagining them separate species.

Owen accentuated the differences for his own ends. He could now demand the total irrelevance of immature ape physiognomy. The flat-face, big-brain, and long legs were circumstances peculiar to its nonage; thus he prised man and ape apart by annulling the middle taxonomic rung created for the immature *Troglodytes*. By separating the terminal rungs he attempted to make the last step impassable; he correctly saw that a taxonomic continuum underpinned contemporary transmutation. He also challenged Lamarckians on specifics; he singled out Bory’s “Orang” article in the *Dictionnaire*, and proved the myological impossibility of an orang standing erect and being counted a man. The problem “unforeseen by Lamarck” was that the *flexor longus pollicis pedis* in man terminates in a single tendon concentrated on the big toe,

and in the orang in three tendons to the middle *three* toes. So that in man it functions to help raise the heel, and in orangs to *grasp* branches

It is surely asking too much to require us to believe that in the course of time, under any circumstances, these three tendons should become consolidated into one, and that one become implanted into a toe to which none of the three separate tendons were before attached. The myology of the Orangs ... affords many arguments equally unanswerable against the possibility of their transmutation into a higher race of beings (80).

What emerges from Owen's article is that transformists and their opponents agreed that *taxonomic continuity* gave the strongest support to the theory of transmutation. Owen's conflation of taxonomic discontinuity and anti-transformism was clearly an adaptation to the contemporary structure of Grantian serial progressionism. Owen never wavered from the line that the taxonomic distinctness of man and ape carried anti-Lamarckian connotations, which explains why he so often juxtaposed the two issues, talking, e.g., of the "unfailing and impassable generic distinctions between *Man* and the *Ape*" (81). He also hit out at the recapitulationist teratological assumptions of Geoffroy and Grant, denying that degradation of humans altered their essential nature. Even a retarded human cranium retained specifically human characters. In the idiot's cranium the brain might have been arrested at about the size it occupies in apes "and where the intellectual faculties were scarcely more developed", yet it was undeniably a *human* cranium. So humans cannot be bestialized,

any more than apes can transcend their station: “Those features of the *cranium* of the *Orangs*, which stamp the character of the irrational brute most strongly upon their frame, are ... of a kind, and the result of a law originally impressed upon the species, which cannot be supposed to be modified under any circumstances ...” (82).

If Owen’s conclusions were more an ideological overlay than empirical deduction, then a rival transformist could obviously have found a different ‘meaning’ in the same evidence. The following year (1836) Geoffroy discussed the developmental differences between man and ape in two papers. He conceded that the young orang’s cranium resembles a child’s, and that an “overgrowth” of the skull and brain retardation leave it with “une bestialité révoltante” (83). He too worried over human dignity (proving that this was not the prerogative of the more sensitive English) but warned against resorting to “theocratic inspiration” to stifle progressive opinion. Moreover his language suggests how he might have seen these same facts in a rival light: he compared, for instance, the distance between young and old orangs with that between the genera *Canis* and *Ursus*, and marvelled at finding such ontogenetic differences which “reveal the facts of a successive development in a single species” (84).

3) The Palaeontological Argument Against Continuous Ascent

Owen’s strategy had professional pay-offs within the

zoological community. His defeat of the greatest living morphologist, Geoffroy, and repudiation of bestializing transformism, coincided with the conservative victory at the ZS elections of 1835. This had strengthened the hand of Tories on the Council, making it more representative of the interests of the society's backers. As the reformers retreated and the political outlook in Bruton Street swung to the Right, Owen's grip on the Society's research material increased. He went from strength to strength until by 1840 he was given a virtual monopoly in terms of dissection. His theistic conclusions concerning man's modification to receive an immortal soul also chimed well with the Coleridgean interests of the RCS and would have been welcomed by elite surgeons suspicious of the secularism of working-class radicals.

His approach appealed to the clerical dons of the ancient universities. By 1833 Owen was already making experiments for William Buckland. Two years later the Owenses honeymooned at Oxford and Buckland was tapping Owen's brains on marsupial generation and the Stonesfield 'opossum' (85). But until the mid-1830s Owen concentrated on consolidating his position in Bruton Street, and had yet to interact in a major way with the gentlemen of the Cambridge Movement. Only after his election to the Hunterian Chair at the RCS (1836) do we find him receiving extensive patronage from Whewell and Buckland, in their capacity as managers of the BAAS and GS. He then began to exploit this social acceptance by canvassing for

increased funding. Historians have provided a number of perspectives on the institutionally-powerful Oxbridge Anglicans. The Cambridge educationalists, science cultivators, historians, and mathematicians, constitute, in S.F. Cannon's analysis, a 'network' defined by a common set of social goals. Its members fostered 'liberal' theology, Romantic poetry, and 'conservative' educational reform (86), as well as mathematics and natural science. One of the network's more interesting 'nodes', as Cannon calls them, was the Germanizing tendency of the Apostles and the lionizing of Coleridge. An Apostles-based "band of Platonico-Wordsworthian-Coleridgean-anti-Utilitarians" gained control of the *Athenaeum* in 1828 and used it "to fight the dragon of materialism" (87). This Coleridgean connection allows a direct interlocking with the Lincoln's Inn gentlemen who influenced Owen. J. H. Green taught Owen a form of conservative romanticism, and Owen's anti-materialist anatomy in turn found 'Network' sympathisers in Sedgwick and Whewell. (These dons were already showing disdain in the 1830s for Paley's utilitarian moral philosophy. This was partly on account of its appropriation by Benthamites, who were hated by Cambridge Whigs and Peelites for their cavalier attitude to "the safeguards of the established order" (88).)

To understand the mechanics of Owen's patronage by members of the Cambridge Movement, we need to focus on their control of the purse strings in the BAAS (f. 1831). This had been dominated, stabilized, and made an organ of 'network'

philosophy only a short time after what Frank Turner calls its “Provincial, cantankerous, and anti-establishment” birth (89). Morrell and Thackray in *Gentlemen of Science* have depicted the Association being moulded, like so many contemporary societies, into a comfortable coalition of gentry and prosperous middle classes at a time of working-class unrest. It fostered “national identity, common commitments, and a continuous acceptance of the leadership claims of traditional rulers”; and since it appealed to ‘neutral’ nature to legitimize its social claim, it promoted an image of ‘scientists’ as outside the political sphere, actually constituting, in T. S. Traill’s term, a “*Fourth Estate in the Realm*” (90). Morrell and Thackray reconstruct the BAAS as an incarnation of Coleridge’s clerisy: a wing of the National Church comprising clerics, dons, and professional and landed supporters almost all with ties to the ancient universities.* The managers thus related civil and natural order and portrayed them as Divine institutes (92); and they interpreted nature in terms of Anglican theology and Coleridgean idealism. They were suspicious of factions to the left of

* One has to be careful not to overstate the case for homogeneity. There was limited grass-roots radical support and Radical MP’s like Warburton and Ewart were enrolled as Life Members. The conciliatory appeal to nature was after all a powerful one. At the other extreme, high Tories were a small but no less significant minority, and Lyell deliberately downplayed the Association’s ‘liberal’ aims so as not to alarm them (91). The political differences *inside* the Association would itself make an interesting study. Whatever these minority interests, though, the *ruling oligarchy*, was socially uniform.

Brougham (who was himself known to favour the distancing of the Whigs from Parliamentary Radicals (93)); and an imperial gentleman like Murchison actually tried to bar mechanics from one meeting. They shunned sciences like phrenology with social-reformist pretensions, and abhorred Continental materialism. Their propagandist image of science was “as an intellectual progenitor of technology, the guarantor of God’s order and rule, the proper way of gaining knowledge, and the key to national prosperity and internal harmony” (94). It was an appeal to the Divine wisdom of the existing social structure and a warning to insurrectionary working men.

The managers encouraged the kind of science which underpinned their social, religious and political views. As Turner says, they could designate who were to be regarded as the proper practitioners and spokesmen of science (95), and one way to determine who they regarded as acceptable is by studying their channelling of funds. Owen’s providential, anti-transformist anatomy met their standard exactly, and as a result he was a leading beneficiary of BAAS patronage. According to Morrell and Thackray’s calculations, he figured among the top six individuals to receive grants-in-aid between 1833 and 1845 – taking £618 or five per cent of the total during these years (96). Even this statistic belies his real worth since he did not start receiving grants until 1838 (for a Report on British fossil reptiles). (Indeed, he did not read his first paper, on marsupials, until that year.) In 1838-45, therefore, his percentage was staggering by any

standard. This is especially true when one considers that most of the other major awards were for practical, naval, geographical, or navigational research. In cash terms, the value of his science to the Anglican elite was rated on a par with navigational-geographical research of the greatest *practical* benefit to a colonial nation. The unabashed partiality of the managers often showed, as when Murchison advised Owen “completely between ourselves to give me such a *report* of the state of that branch of our knowledge as you would read if you were General Secretary commenting on *Professor Owen’s Memoir* … My sole object is to give you the widest possible extension to your views and *wishes*, in order that the *good* which the Association does may be *felt* and *acknowledged*” . Owen’s value can be understood on social, cultural, or nationalistic levels: social because of his anti-radical support for the traditional political rulers, cultural for his Coleridgean idealism which could ensure the hegemony of the National Church, and nationalistic because he was establishing Britain’s reputation in the eminently French preserve of comparative anatomy: not, like Grant, by emulating and developing French philosophical thought, but by pursuing an independent British course.

Association business flourished. Profits for 1837 finally topped £2000 as a result of a massive rise in Life and Annual Membership receipts. Consequently, amounts paid out in grants rose 71 per cent in 1838 and over the next five years averaged £1480 p.a. (97). Owen was perfectly placed to reap

the benefit; his lump-sum grants were large by comparative standards. The Geology Committee comprising Greenough, Lyell, and Clift (Owen's father-in-law), put £200 at his disposal in 1838 to enable him to visit collections and draw up a report on British fossil reptiles (98). Records show that the following year payments to further this end totalled £118. 2s. 9d (99); and on completion in 1841 the managers allotted a further sum of £250 to publish the report (100). This massive injection of funds amounted to a vote of confidence, making it professionally expedient for Owen to present his most damning case against Lamarck, Geoffroy, and their British disciples at the forum provided by the Association.

What prompted Owen's move into palaeontology can best be gauged by a study of MS sources. I do not think he was passively led to it by, say, Hunter's fossils in his own museum, or that he was necessarily following a Paris-Edinburgh tradition (where comparative anatomy had always been closely linked to fossil zoology). Nor do I think that nationalism was necessarily the original motive. However it is true that Owen's anti-transformism was a stalwart barrier to Continental atheisms. And of course it was welcomed by the Association's aristocracy as a powerful defence of God and the divinely-instituted social order. Notwithstanding the internationalist pretensions of the early BAAS, the body is better understood as "HER MAJESTY'S PARLIAMENT OF SCIENCE" (101), Trail's constitutional metaphor which acknowledged the

BAAS's governorship of a *national* body. As 'parliamentary' members, the managers were naturally concerned with sovereignty, often with British priority in the scientific field. With priority came prestige; thus it was doubly important for whipping up national pride at home, especially in the wake of the declinist debate. This nationalistic message was clearly spelt out in the Presidential address in 1842 by one of Owen's admirers, Lord Francis Egerton. He saw scientific discovery bestow privilege. While new facts "become the common property of man, for that very reason, and because they confer that common benefit, they elevate the country in which they originate in the scale of nations, and gratify the most reasonable feelings of national pride..." (102). Those higher nationalistic feelings that Owen did entertain, however, were mediated through his repudiation of Lamarckism.

Nor do I think that fear of foreign competition was the instigating factor, although this subject should be raised because the Tory MP for Chester Sir Philip Grey Egerton (1806-1881) did broach the question with him. Egerton was a fossil fish collector and enthusiastic BAAS supporter. He entertained the visiting savants at his Cheshire manor during the Liverpool meeting in 1837 (103). At this time he had only recently returned from a European tour worried by the omnivorous tendencies of German palaeontologists, and suggested that Owen draft a report on British fossils to seize priority. As he later told Owen:

I had just returned from the Continent, where I had an opportunity of seeing what von Meyer, [*illegible*], Münster [*] and others were engaged upon, and so confident was I that a vast field for original [research] in this branch of Paleontology [sic] was offered in our Collections, at the same time so fearful of the harvest being gathered by a Foreigner, and so anxious that you, for whom I have so great a regard, and whose Talents and discrimination I considered so [supremely] fitted for the inquiry, should have the fruits, that I felt myself impelled to take this step, and to follow it up by the second application at Newcastle, to enable you to undertake the preliminaries for your task (107).

Egerton congratulated himself for, “if not causing, at all events of accelerating the production of so valuable a report”. So there probably were pronounced nationalistic reasons for the report being *sponsored*. But Owen’s own *motives* for moving into palaeontology and beginning work on enaliosaur are better indicated by the meaning he gave his conclusions. The notebooks themselves suggest that the primary instigating factor may have been less directly patriotic. It is difficult to escape the conclusion that his first note on the subject more precisely defined his real

* Count Georg Münster (1776-1844) was well known to English geologists. Murchison had described him to Sedgwick in 1830 as “the prince of fine, honest-hearted, intelligent travelled Germans. His cabinet, without any exception, [is] the most instructive in Europe for the oolitic series, and down to the coal-measures” (104). Münster offered his collection for sale in the 1830s (perhaps this is what attracted Egerton to Bayreuth in 1837); and a duplicate collection, which Sedgwick considered “nearly as good as the first, which is unquestionably the finest in all Europe” (105), went on sale in 1839 (Sedgwick persuaded the Woodwardian Trustees to buy it). The managers of the BAAS would have been in no doubt about the potential threat from collectors like Münster; hence Egerton, Murchison, and Buckland were keen to see Owen seize priority (106).

fears. During or shortly after November 1834 he jotted:

fact wholly at variance with every theory that would derive the race of Crocodiles from Ichthyosauri & Plesiosauri by any process of gradual transmutation or development (108).

The entry sits alone. He does not say what the “fact” is; nor is Geoffroy named, although there can be no doubt that Owen has targeted his “*Mémoires sur de Grands Sauriens*” (1833), in which crocodiles are derived ultimately from ichthyosaurs via the recently discovered Caen *Teleosaurus*. Geoffroy had emphasized the transitional nature of the teleosaur’s palatine and skull-roof plates, and had placed his fossil sequence in an explicitly transformist context. In his first paper on the teleosaurs he praised Lamarck’s laws and saw “transmutation and metamorphosis” everywhere in nature (109). Between October 1830 and August 1831 he read five memoirs on the “grands Sauriens” to the Academy, the fourth dealing with the environmental influences causing the teleosaur’s transformation (in which he again mentioned Lamarck’s laws (110)). The fifth memoir was delivered on 29 August when Owen and Grant were together in Paris. Geoffroy never lost interest in the subject. In September 1836 he made an (abortive) trip to London, both to bring Grant a paper on the hyoids and search for teleosaur paddles in the Oxford Secondary deposits (111). Nor was Geoffroy any quieter on the subject of transformism. In the following year during a heated debate with Blainville over Falconer and Cautley’s Indian fossil *Sivatherium*, he made his famous claim that the

age of Cuvier was closing and belief in the immutability of species was on the wane (112).

Owen's notebook suggests that he was reacting to Geoffroy's *Mémoires*, and the strongly worded conclusion to his Report on British fossil reptiles also implicates Grant, from whose *Lancet* lectures Owen quoted "the latest terms in which the transmutation-theory has been promulgated, as supported by palaeontology," (113).

In 1838 Grant was Owen's substitute as Fullerian Professor at the RI; he was shortly to publish his palaeontological *General View*, copies of which were struck off and sold by Bailliere for 3s.6d. This we know caused consternation among the GS elite (next chapter); and in it Grant appealed as usual to Geoffroy's 'unity' and applied Blainville's serialism to the fossil record, making life a continuous succession. Owen understood the transformist meaning of Grant's fossil 'continuity'. And it was Grant's belief in "gradual transitions" that he now set about undermining.

Because Owen's opposition to Grantian progressive development was well established before he set out on his BAAS-sponsored tour of the major collections at home and abroad (114), he could marshal evidence specifically to refute Geoffroy's ichthyosaur-teleosaur sequence. Indeed, the famous enaliosaur fossils of the eccentric Glastonbury poet and Scriptural geologist Thomas Hawkins (1810-1889), some of the best at Owen's disposal (Hawkins boasted that his "transcends all the collections in the world" (115)), were

possibly not available for study until 1835, when the collection was unpacked and displayed at the British Museum (116). The notebooks show Owen only beginning to collect data methodically late in 1837, i.e. after Egerton had raised the subject at the Association. The size of Owen's subsequent funding was only exceeded by the length of his report, the two parts of which took a total of five hours to read. At Birmingham in 1839 he described twelve new species of plesiosaurs, and six new ichthyosaurs (117), which put him in a commanding position to tackle the transformists on stratigraphic grounds. Only by looking at his conclusions, i.e. to the *work* he expected his fossils to do, can we grasp the real 'meaning' of Owen's studies. He wound up at Plymouth in 1841 using the saurian distribution through time as an explicit test of transformism. He asked:

To what natural or secondary cause, it may then be asked, can the successive genera and species of Reptiles be attributed? Does the hypothesis of the transmutation of species, by a march of development occasioning a progressive ascent in the organic scale, afford any explanation of these surprising phaenomena? Do the speculations of Maillet, Lamarck and Geoffroy derive any support or meet with additional disproof from the facts already determined in the reptilian department of Palaeontology? (118)

He conceded that superficially fossils might appear to lend support to transformism, "but of no stream of science is it more necessary, than of Palaeontology", he warned referring to Grant, "to 'drink deep or taste not'".

Owen tested the transformist hypothesis against the

“poikilitic strata” in two ways. First he subjected Geoffroy’s sequence to scrutiny. He stressed that the *Ichthyosaurus*, with both inferior ichthyic and superior cetacean features, was just the animal, “were specific forms unstable, to demonstrate a mutation of characters or some tendency towards a progressive development into a higher and more consistent type of organization” (119). But he insisted that it can be traced “generation after generation, through the whole of the immense series of [Secondary] strata” without showing any noticeable change. “*Ichthyosaurus* quits the stage of existence as suddenly as it enters it in the lias ... with every appreciable osteological character unchanged”. Different species appeared simultaneously, and there was no evidence of *succession*. The same was true for genera; only if they were reshuffled chronologically could any sort of succession be inferred. in that case it would be “as easy as seductive to speculate on the metamorphoses” by which an ichthyosaur developed itself into a plesiosaur and thence a crocodile, and we would then be sanctioned to investigate the “physiological possibilities of such transmutations”:

Ichthyosaurus, *Plesiosaurus* and *Teleosaurus* are, however, genera which appeared contemporaneously on the stage of vital existence: one neither preceded nor came after the other. How the transmutation theory is to be reconciled to these facts is not obvious; nor to these other, viz. that the *Teleosaur* ceases with the oolite, while the *Ichthyosaur* and *Plesiosaur* continue to co-exist to the deposition of the chalk, and disappear together alike unchanged (120).

Owen's mooting of stratigraphic *ranges* was designed to establish the stability of species through immense periods and thus destroy Geoffroy's transformations. But this empirical approach still lacked the dramatic impact (and therefore cultural usefulness) of his *coup de grace* – the quadrupedal dinosaur – designed with a constitutive anti-Lamarckian component and introduced at the 1841 meeting. From the point of view of cognitive sociology, Owen's 'manufacture' of the *Dinosauria* is instructive. The stratigraphic 'results' could be considered as straightforward refutation (i.e. a more truthful representation of 'reality') – the dinosaur, however; contains an essentially creative, culturally-relevant component, which can be determined from the immediate use to which Owen put it. To revert to the 'instrumental' model: Shapin suggests that we treat the generation and evaluation of knowledge as if goal-directed. The profitability of this approach becomes apparent taking the dinosaur as a case study. It provides one of the best examples of cultural adaptation of a scientific resource. A contextualist study of the episode also acts as a counterpoint to more positivist interpretations. Thus Delair and Sarjeant assume the transcendent reality of the taxon 'Dinosauria'; this allows them to search old records for naturalists perspicacious enough to recognize the fact. Their terminology is particularly revealing: naturalists in the seventeenth and eighteenth centuries were first to describe dinosaur bones but misinterpreted them (121); even Buckland

“missed the opportunity” of describing “a sauropod dinosaur” (Sauropoda was a later nineteenth century American taxonomic creation). This positivist model assumes a gradual illumination dotted with heroic insights and missed chances, until by 1841 enough was known for Owen to step in and provide a convenient label for this group – the word ‘dinosaur’.

Of course I have raised a straw man. Few historians nowadays accept such Whiggishness; the point is to provide a more satisfying cultural explanation. On the instrumental model, ideas and interests are contextually-mediated, such that social groups in given cultural loci may find ideas useful in furthering their particular ends. Barnes talks of “ideas as tools” (122). But ideas embodied in more concrete form, as in the case of reconstructions, make equally effective tools. Indeed the dinosaur is a singularly good choice for study; with so few fossil bones known, its restoration entailed use of cultural imagination; this is what makes Owen’s ‘invention’ so revealing. It looked nothing like the “Fossil Lizard” of his predecessors Buckland and Mantell – although it was *their* evidence that he largely reinterpreted – partly because it was refashioned for different ends. So I would turn Delair and Sarjeant on their heads and start with Owen’s *cultural* creation. In an extreme formulation (which might ultimately rest on the case made by Berger and Luckman in *The Social Construction of Reality* (123)), it could be mooted that the ‘dinosaur’ did not actually exist prior to 1841, and was only brought into

existence then partly to serve social ends and that its success on this level helped establish its scientific worth. (At least it remained unmodified until a new social group – Huxley’s “plebeian” mercantilists – got hold of it in the 1860s and rebuilt it for Darwinian ends.)

My aim is to show how Owen recast the gross morphology of the “Fossil Lizards” and convinced his Association audience – including Sedgwick, Buckland, and Conybeare (124) – of its value in crushing the transmutationist heresy. First we should look at the details of the paradigm switch. Following Cuvier’s diagnosis of the Dutch *Mosasaurus* as an extinct monitor, English geologists in the 1820s fitted their own newly-disinterred saurians into the same lacertilian mould. Buckland announced his Stonesfield “Megalosaurus or Great Fossil Lizard” to the GS in 1824; Gideon Mantell in 1825 reinforced the diagnosis by announcing the discovery in Tilgate Forest of the teeth of a giant extinct reptile – so like those of the diminutive iguana in the RCS that he coined the name *Iguanodon* (125). The *Megalosaurus* was very imperfectly known. In 1824 the Oxford museum did not even possess two articulated bones apart from some vertebrae, and the disarticulated bits and pieces came from different individuals. Nonetheless Buckland confidently announced that the parts “are yet sufficient to determine the place of the animal in the zoological system” – viz. in the “order of Saurians or Lizards” (126). This diagnosis was of immense

practical value, since scaling up and size estimates could follow. Thus the largest femur in the Oxford museum, at two feet nine inches, led on the lizard model to a projected length of the megalosaur of over forty feet. This was far from preposterous, but larger bones gave more unwieldy results. Thus Buckland estimated from a large femur in Mantell's collection that, assuming

the total length and height of animals were in proportion to the linear dimensions of their extremities, the beast in question would have equalled in height our largest elephants, and in length fallen but little short of our largest whales ...

although Buckland and Conybeare noted that the ratio of body to leg length decreases with size and so they settled on a more manageable "sixty to seventy feet". This method of calculation was accepted by Cuvier, Buckland, Mantell, and Conybeare. Their caution was not for fear that the *model* was wrong, but stemmed from the knowledge that the proportions of living lizards varied (127). By the late 1820s Mantell was predicting a length of seventy feet for his *Iguanodon*; while Buckland in 1829, describing a "gigantic metacarpal bone" from the thumb of a reptile, claimed that

if we apply to the extinct animal from which it was derived, the scale by which the ancients measured Hercules ("ex pede Herculem"), we must conclude that the individual, of whose body it formed a part, was the most gigantic of all quadrupeds that have ever trod upon the surface of our planet (128).

Owen was sceptical of the exaggerated lengths claimed using the lizard model. He reckoned that the largest bones scaled-

up would yield fantastic sizes, upwards of 200 feet. Moreover, a few geologists were beginning to postulate non-lacertian adaptations to these enormous sizes: Buckland recognized the necessity of thicker wrist bones in megalosaurs to take the weight (129), while the prolific German amateur Hermann von Meyer in 1832 categorized these Secondary saurians as having limbs like “those of the heavy Land Mammalia” (130). So morphological reinterpretation was beginning on a small scale; but not such as to warrant the sweeping changes in morphology and physiology Owen now proposed.

How Owen rendered a gross morphological transformation was straightforward. To reduce snout-to-tail length to a manageable 30 feet for *Megalosaurus* he measured individual vertebrae and multiplied by their estimated total number (derived from a lizard model). But his shortening of the trunk necessitated dramatic changes in stance. The limbs by comparison now assumed mammalian proportions; and since such weight militated against a sprawling gait, Owen swung the legs under the torso, giving megalosaurs a rhinocerine aspect. The analogy is not far fetched, for his repeated comparisons with “pachydermal” mammals was integral to his strategy. Throughout he emphasized the osteological similarities with “heavy pachydermal Mammals” (131). More intriguingly, he extended the comparison to physiology and ecology. Speaking of soft anatomy, he hazarded that

The Dinosaurs, having the same thoracic structure as the Crocodiles, may be concluded to have posses-

sed a four-chambered heart; and, from their superior adaptation to terrestrial life, to have enjoyed the function of such a highly-organized centre of circulation in a degree more nearly approaching that which now characterizes the warm-blooded vertebrate (132).

These were extremely speculative statements, given the paucity of fossil evidence, and Owen knew it:

A too cautious observer would, perhaps, have shrunk from such speculations, although legitimately suggesting themselves from the necessary relations between the organs and media of respiration; but the sincere and ardent searcher after truth, in exploring the dark regions of the past, must feel himself bound to speak of whatever a ray from the intellectual torch may reach, even though the features of the object should be but dimly revealed.

Nonetheless Owen persisted in reconstructing an immense multi-ton “pachyderm”, built to rhinocerine specifications and boasting an almost mammalian circulation. He had moved a long way from the image of the “Fossil Lizard”; but he needed to project the dinosaur as the apotheosis of the reptilian condition, “rejoicing in these most perfect modifications of the Reptilian type” – and as “superior in organization and in bulk to the Crocodiles that preceded them as to those which came after them” (133). In order to focus attention onto the group, he dignified them with a title and rank befitting their station – he created the evocatively titled new order *Dinosauria**.

*Owen's pachydermal dinosaur was commercially marketed in 1854, bringing it to a mass audience. The profits made by the Crystal Palace Company during the Great Exhibition in 1851 were ploughed back into animating the antediluvian world at Sydenham in 1854. Here Sir Joseph Paxton's glass-curtain

Owen now *used* the dinosaur as an empirical test of the truth of Grant's claim – that fossils appeared in serial succession. Obviously if progressive development “ever extended beyond the acquisition of the mature characters of the individual, so as to abrogate fixity of species by a transmutation of a lower into a higher organization” (140), then lower forms ought to be found in older strata. But the Magnesian conglomerate and New Red Sandstone, instead of entombing the “fish-like perennibranchiate Batrachians” (permanently-gilled Amphibia like *Proteus*), actually house

Crystal Palace was to be re-erected; and Owen apparently suggested to Sir Joseph that the grounds be adorned with the more spectacular British saurians (134). Owen advised the Directors of the CP Company that the artist and sculptor B. W. Hawkins be engaged (he had used him already as an illustrator), although Owen remained wholly responsible for the design details. (Paxton recommended Owen for an Architect's percentage but he declined, so the Directors presented him with a 'Life-Admission' pass instead (135).) Thus life-size concrete and steel restorations of Owen's mammalian dinosaurs became celebrated among the foremost Victorian “arts of the nation”. Hawkins told the Society of Arts (the grandparent of the CP) that the enterprise of the joint-stock Company would benefit all classes, adding that it was no less a benefit “because it was done commercially, which, if properly analysed, will be found to be the most truly independent system and most congenial to the feeling of every right-minded Englishman” (136). But the dinosaurs in this “mausoleum to the memory of ruined worlds” (137) were mostly intended as sober instruction for the labouring classes, for whom special trains were laid on. The *Westminster* pictured them gawping at the monsters in blissful Noachian ignorance; the Tory *Quarterly* saw the artisans so well turned out that they could barely be told from gentry, drawing the comfortable conclusion that life's benefits “must be widely spread in a land which can send out such a *mob*” (138). Hawkins subsequently mass produced miniatures for schools and “Parochial Institutions”. Manufactured at one inch to the foot, they sold for seven guineas per set of seven (139). So Owen's dinosaur restorations were commercially successful and financially beneficial to the shareholders of the Crystal Palace Company. As a self-propelling propagandist vehicle for his ideas, the whole enterprise could hardly have been bettered.

the *highest* batrachians, the armoured labyrinthodonts (which, in the “figurative language of the transmutation theory” were better described as “degraded Crocodiles”). The alleged fish-like origin of the reptiles was even “more directly negatived” by the presence of thecodont *lizards* in rocks more ancient. Lizards older than amphibians controverted the idea that reptiles “emerged, by progressive development of structure, from any lower organized pre-existing group of cold-blooded animals”. At the other end of the scale if transmutation be true each class ought today “to present its typical characters under their highest recognized conditions of organizations” (141). Owen had engineered his new dinosaur, with its quasi-mammalian cardiovascular system, as a definitive refutation of this. As an immense “Pachyderm”, his dinosaur was osteologically superior to any sprawling reptile; it played the most dominant ecological role “that this earth has ever witnessed in oviparous and cold-blooded creatures”; and in terms of Lamarck’s own cardiovascular criteria (142), it had reached the apotheosis of the reptilian condition. Yet the dinosaurs had peaked in Secondary times. Just as labyrinthodonts had dwindled into diminutive frogs, so reptiles had “now subsided into a swarm of small Lacertians” (143). This degeneration *within* classes was the key. The classes themselves might make a regular ascent, but within them there was no uninterrupted contiguity, with the highest of one class abutting on the lowest of the next (as Grant demanded). Quite the reverse. Reptiles did not begin in fish-like simplicity, “nor have

they terminated at the opposite extreme, viz. at the Dinosaurian order”, where they appeared most mammalian. The “sudden extinction” of dinosaurs and “abrupt appearance” of mammals heralded the end of the age of reptiles, yet neither event was explicable by “any known natural causes or analogies”. Nor was there evidence that dinosaurs had mutated *into* mammals: the appearance of tiny oolite “opossums” had not only *preceded* the dinosaurs’ demise (by Owen’s reckoning: Grant and Blainville disagreed), but they could hardly, “by any ingenuity or licence of conjecture”, be derived from them “without violation of all known anatomical and physiological principles” (144). From the suddenness of events he drew one main conclusion to comfort the dons:

the interruptions and faults, to use a geological phrase, negative the notion that the progression has been the result of self-developing energies adequate to a transmutation of specific characters; but, on the contrary, support the conclusion that the modifications of osteological structure which characterize the extinct Reptiles, were originally impressed upon them at their creation, and have been neither derived from the improvement of a lower, nor lost by progressive development into a higher type.

Owen and the Gentlemen of Geology

The worth of the ‘dinosaur’ would have been apparent to the dons and landed gentlemen visiting Plymouth. It is probable that the degeneration argument was given greater prominence after Owen’s spectacular application in 1841. The strategy had actually been known a long time. We have already

seen Greenough employing it in 1819 and Fleming in the 1820s; although it was not used extensively except in a Lyellian context (145). Buckland did refer to it periodically in *Geology and Mineralogy*, where his intent was to confront the theory of “gradual transmutation” with the “phenomena of Geology” (146). He earmarked examples of “*retrograde development*” or *Retrocession* among sauroid fishes, cephalopods and crinoids, all of which were high born and therefore attested to “the direct interposition of repeated acts of Creation” (147). Owen made no better use of the strategy in his 1837 Hunterian course; in his third lecture (11 May 1837) he took two examples of retrogression from Buckland’s Bridgewater (noting the early appearance of high-ranking sauroid fish and chambered cephalopods), making the quiet observation that “the different organized forms which have succeeded each other do not display regularly successive stages of complication, or perfection of Structure”.

Plants and animals exhibiting different degrees of complication of Structure have co-existed at different periods; their existence, and fertility, and well-being seems, as now, to have been regulated by the [external] conditions ... ; [after their extinction] new species appear on the stage, endowed with powers and forms adapted to the new conditions of the external world, but not necessarily superior in their Organization to the extinct Species which they have replaced (148).

Owen’s tame statement in 1837 contrasts with his aggressive application of the principle in 1841. He was now aware of the strategic value of the ploy and prepared to press it into service. His tactical alignment with Buckland and brushes

with Grant at the GS in 1838-9 may partly explain this, but also his first real contact with palaeontological data after 1837 must be taken into account. Owen enhanced the status of the degeneration argument, and his deployment of the “pachydermal” dinosaur revealed its immense potential. Where before the strategy had largely been employed in a Lyellian context, ‘degeneration’ was now brought to the service of *progressionists* like Sedgwick, Buckland, Conybeare, and Whewell, each in his “double capacity as divine and saurologist” (149). We have still to judge the influence of Owen’s ideas and their reception by the Anglican and wider geological community, before using this to explain his personal and professional aggrandisement. First, though, we need to understand the preconceptions entertained by geologists in 1841.

Modern scholars, from Cannon and Gillispie to younger historians like Rudwick, Bowler, Bartholomew, and Lawrence (150), have emphasized the reaction of the ‘directionalist’ orthodoxy to Lyell’s *Principles*, and the *need* felt by clerics like Sedgwick and Conybeare to negate Lyell’s steady-state doctrine in the 1830s (151). The dons defended a diametrical belief, as Sedgwick put it in the GS address in 1831, “that the approach to the present system of things has been gradual, and that there has been a progressive development of organic structure subservient to the purposes of life” (152). The response to Lyell was certainly pronounced. However much Lyell had “abused” progressive development, Sedgwick still considered it sound

and founded on “toilsome inductions (153). Conybeare too was adamant; he told Lyell in 1841 that the record revealed “a converging series from the most to the least perfect of the Vertebrata” (154). Lawrence, interested in Elie de Beaumont’s rival dynamics, sums up the situation as Conybeare perceived it: “the progressive global climate and faunal populations to the evidences of decreasing tectonic activity and the progressive patterns necessitated by an incandescent planetary origin, pointed to a directional interpretation of earth history” (155). Murchison even tried to heal his breach with Sedgwick by appealing to common philosophic ground – pleading that at least “we agree in the grand doctrine of a progression of creation” (156). Buckland, though never denying that instances of “*Retrocession*” were “fatal to that doctrine of *regular Progression*”, nonetheless affirmed that *overall* fossil creatures “were constructed with a view to the varying conditions of the surface of the Earth, and to its gradually increasing capabilities of sustaining more complex forms of organic life, advancing through successive stages of perfection” (157). Since one of his objectives in the Bridgewater was to dispose of Hutton and Lyell, his ascending scale was actually composed of “near and gradual connexions”, and the series with its incontestable unity proved the “one grand original design” of creation (158). Lyell of course never doubted a wise design but *his* design* argument differed

*Design arguments served a mediating function, not only between doctrinal sub-cultures, as Brooke has shown, but

from Buckland's. For the latter unity *and* directionalism were necessary to prove Unity of Intelligence.

So Lyell's colleagues at the GS rejected his non-directional palaeontology, and many continued to advance views of the gradual ascent of ancient life. As Bartholomew says: "they failed to recognize the anti-evolutionary strategy that he was offering them" (160). But they were not insensitive to the transformist threat. After Lyell published *Principles*, the Oxbridge dons were if anything *more* aware of the Lamarckian problem. One has only to read the reviews of the first volume (1830), in which Lyell broached the "undeviating uniformity of secondary causes". (He did not tackle Lamarckism until the second volume.) Lyell's apparent naturalism alarmed Sedgwick, who saw how continuously active secondary causes would pave the way for infidel speculations:

I may remind you, that in the very first step of our progress, we are surrounded by animal and vegetable forms, of which there are now no living types. And I ask, have we not in these things some indication of change and of an adjusting power altogether different from what we commonly understand by the laws of nature? Shall we ... adopt the doctrines of spontaneous generation and transmutation of species with all their train of monstrous consequences? These subjects, indeed, are not yet touched upon by Mr. Lyell; and I throw out these remarks only to show by what difficulties the Huttonian hypothesis is encountered ... (161).

possibly in uniting religious groups opposed to radical unbelievers. Wakley for example yawned at Charles Bell's gratuitous efforts to prove design in his Bridgewater (159); and Grant laughed at Providence. Lyell and Buckland, whatever their scientific differences were united in their *designful* opposition to infidelity.

Whewell too was struck on reading the first volume by the difficulties presented by a naturalistic ‘uniformitarian’ programme. He charged that

By thus asserting the former states of the earth and sea, which geological inquiry presents to us, to be parts of one continuous progression, carried on by the same causes which act at the present day, the author puts himself under an obligation to show:-...That the changes from one set of animal and vegetable species to another, are also explicable or conceivable on the assumption of the same conditions (162).

Geological naturalism was one thing; Sedgwick’s and Whewell’s point was that to retain a shred of consistency it would have to be applied to organic nature as well. In his *Quarterly* review of volume two, Whewell congratulated Lyell for “wisely, we think, and philosophically [opposing] himself entirely to the assertors of the geological adequacy of the existing laws of *organic* life” (163). He was referring to Lyell’s refutation of the “physiological laws” employed by transformists to explain changes induced by domestication, acclimatisation, and hybridisation, and which, in the context of fossil life, were supposed to account for the transmutation of species – even for the “production of man” (“all metamorphoses having become to them equally probable” (164)). Whewell then dismissed Lamarckism as hopelessly inadequate, the more insistently because “many of the geologists of France entertain no doubt of the theory of transmutation being that by which the different forms of animal life, at different periods of the earth’s past history, are rightly explained”. The situation remained essentially the same

throughout the 1830s. Like Buckland in his Bridgewater, caught between refuting Lyell's steady-state geology, yet offering nothing to encourage the progressive transformist (165), Sedgwick in his "sledge-hammer against the Utilitarians" (166), the 1835 *Discourse*, dismissed the "phrenesied dream of the transmutationist", yet considered Lyell almost equally "rash and unphilosophical". (Lyell was flabbergasted to see himself lumped with Lamarck as an infidel in the *Norfolk Chronicle*'s report of the lectures! (167).) Whewell too was critical of Lyell but aware of the need for an effective anti-Lamarckian *progressionist* strategy. In his *History of the Inductive Sciences* (1837) he became outspoken because, as he noted, Geoffroy in Paris was celebrating the end of the age of Cuvier (168).

We now have the seeds of an explanation of why it was *Owen* who developed the degeneration argument, and why it was so successful. At the RCS he had become acutely aware of the institutional and social threat posed by the Radical Lamarckian alliance, the more so because his dealings with Grant close by in Gower Street involved an element of professional rivalry. Therefore his main concern was to pioneer the degeneration argument within a progressionist context – designing a rival to Lyell's strategy within a directionalist framework. Senior geologists at the GS and BAAS like Buckland, Sedgwick, and Whewell were receptive to any strategy meeting these specifications. More so than I

formerly suspected; in “Designing the Dinosaur” I suggested that Buckland, not being permanently engaged in London, was less aware of the immediate danger of Grantian materialism. But I was wrong; MS evidence to be presented in the next chapter shows that he and Owen *conspired* at the GS in 1838-9 to outmanoeuvre Grant on a question intimately concerned with fossil ascent. So Buckland *was* aware of the Grantian threat. This reinforces my argument that Owen’s 1841 strategy would have been immensely appealing to the savants of the BAAS, no less than to landed gentry like Sir Philip Egerton or the Earl of Enniskillen, whose manor houses were replete with expensive cabinets.

Owen was ideally placed at the BAAS to denounce Radical sympathising Geoffroyan materialists – especially in 1841 against a backdrop of economic gloom and Chartist agitation. The Chartists had rioted in Birmingham in 1839 only a month before Owen read his first report on British saurians there. The week of the meeting itself was a “feverish quiet” with peace ensured by “men in green and men in red, police staves and cavalry sabres” (169). On the other hand, the Coleridgean clerisy so congenial to Owen was anathema to those who actually had to earn a living in the “lecture bazaars”; Coleridge’s romanticism might have appealed to the “Church and State bigots” (170) but it was opposed to the interests of radical reformers and the working class unions. Even Brougham and Babbage were too progressive for the BAAS managers (171). Old Corruption Tories and fierce democrats

were made unwelcome at the Association; the emphasis remained on wealthy gentry, leisured gentlemen, and conserving reformism. Buckland and Whewell were Peelites. (Owen probably was too at this time.) Even a Whig like Sedgwick – who held Fox as his hero, and could vote (against Cambridge tradition) for Catholic emancipation and the abolition of religious tests (172) – warned against any “movement of the [Whig] machine” which might let the “radical party gain a cog” (173). And while he thought that it might be instructive for BAAS gentlemen at Manchester in 1842 to *mingle* with the “humbler orders”, he held “levelling doctrines” to be frankly dangerous, resting as they did on a futile faith in the impermanence of barriers “between rank and rank” (174). The deliberate distancing from rank-and-file radicals, and dislike of utilitarians and lecture-hagglers, resulted in many London savants, particularly from the Godless College, boycotting the BAAS. Some who shunned the first meeting later came round (Lindley); others like Grant steadfastly refused to attend. As late as 1850 he told Mantell, “I have never been at a Meeting of the British Association, and do not think they conduce to the advancement of science, or to the interest of our other scientific institutions” (175). Wakley was present at Dublin in 1835, but seems to have misjudged the Association’s potential as a force for liberalism. Or rather, he used the occasion as another excuse to launch a tirade against the contemptible monopolists and legislators; believing that such “*ex-officio* rulers of the human race”

would be humbled by the savants into appreciating the error of their “pernicious adherence to impolitic laws and corrupt institutions” (176).

Given the Association’s largely conservative-Anglican composition and the managers’ use of natural theology to underwrite social stability, it is not surprising that Owen’s anti-Lamarckian gambit paid off so handsomely. He was preaching to the converted: a socially-eminent lobby which abhorred French atheism and made materialism the well-spring of social discontent. Dissidents were barred, and Owen offered a new grindstone to hone an old political axe. For this he was praised by the Association managers. Sir Philip Egerton called Part I of the report “glorious”:

I can only say that I feel no further regrets at having been the cause of imposing this burden on you, and shall always consider that, of my humble efforts in furtherance of Scientific Knowledge, the most important has been, if not causing, at all events of accelerating, the production of so valuable a report (177).

Fellow Conservative member in the House, Lord Francis Egerton, was equally impressed. In his Presidential address in 1842 he lapsed into a paean of praise

See how the greatest – am I wrong in calling him so? – of the British disciples of Cuvier walks among the shattered remnants of former worlds, with order and arrangement in his train. Mark how, page after page, and specimen after specimen, the dislocated vertebrae fall into their places, – and how the giants of former days assume their due lineaments ... all alike bearing the indelible marks of adaptation to the modes of their forgotten existence, and pregnant with the proofs of wisdom and

omnipotence in the common Creator (178).

The imperial geologist, Roderick Murchison, was proud to see English masonry used to complete Cuvier's temple of nature; and he congratulated the Association for *creating* the man of the hour: “when we solicited Professor Owen to work out the subject, we did not follow in the wake of Europe's praise, but led the way (as this Association ought always to do), in drawing forth the man of genius and worth” (179). Not only was Owen's scientific strategy “entirely acceptable” (180) to Association managers, but by actively nurturing him they had ensured a correct ideological stance on Providence and design, and reinforced his vision of nature devoid of “self-developing energies”.

The effect of the managers' patronage was to help create a man in their own image. Members were encouraged to lionize Owen. At Birmingham in 1839, Owen reported to his wife, “Instead of the ordinary applause, all the Audience at the suggestion of the Chairman rose to testify their thanks” (181). And such was the success of the reptile project that the managers in 1841 voted another £200 to enable him to draw up a fresh report on British fossil mammals (182). The glowing press these reports received helped to reinforce the Association's dominant ideology – while their *contents* provided a fundamental resource enabling managers like Sedgwick (and guests from north of the border like Hugh Miller) to check the heretical *Vestiges* a few years later (183).

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130. H. von Meyer, "On the Structure of Fossil Saurians", *Mag. Nat. Hist.*, 1 (1832), 281.
131. Owen, op. cit. (58), 103, 203, 110, 202.
132. Ibid., 204 note.
133. Ibid., 200.
134. That it was Owen's suggestion is evident from his letter to the editor of *Nature*, a MS copy of which is

housed at BM(NH), OC Vol. 21, f. 25; but cf. *New York Times*, 27 February 1870, p. 6, where B. W. Hawkins suggests that the idea originated with Prince Albert and Edward Forbes (the Prince – by now a friend and admirer of Owen – did visit Hawkins' studio in the grounds during the modelling). On the models see Owen's handbook, *Geology and the Inhabitants of the Ancient World* (London, Bradbury & Evans, 1854).

135. Owen's letter to *Nature*, ibid.
136. B. W. Hawkins, "On Visual Education as applied to Geology", *Journal of the Society of Arts*, 2 (1853-4), 444-9 (444).
137. *Quarterly Review*, 3 (1854), 238.
138. Ibid., 239; *Westminster Review*, 6 (1854), 540-1.
139. Hawkins, op. cit. (136), 447-8; details of prices etc. from an advertising broadsheet, "Waterhouse Hawkins' Restorations of Extinct Animals" in the possession of Prof. Stephen Jay Gould, Harvard University.
140. Owen, op. cit. (58), 197.
141. Ibid., 200.
142. Lamarck, op. cit. (12), i, 336-44.
143. Owen, op. cit. (58), 201.
144. Ibid., 201, 202.
145. Buckland, op. cit. (1), vii. [This should be note 146 (whose note number is missing in the original). There is no note 145. AD]
147. Ibid., 294-5, 312-3, 431.
148. R. Owen, "Nature and Character of Organized Beings", Hunterian Lecture 3, 11 May 1837, Manuscript Notes, and Synopses of Lectures. 1828-41, BM(NH) OC 38, ff. 34-5.
149. [W. Whewell], "Lyell's Geology", *Quarterly Review*, 47 (1832), 103-32 (117).
150. On Conybeare's reaction: P. Lawrence, "Charles Lyell versus the Theory of Central Heat: A Reappraisal of Lyell's Place in the History of Geology", *J. Hist. Biol.*, 11 (1978), 101-28 (114-7); on Murchison's, M. Bartholomew, "The Non-Progress of Non-Progression: Two Responses to Lyell's Doctrine", *Brit. J. Hist. Sci.*, 9 (1976), 166-74; on the general progressionist answer to Lyell see: P. J. Bowler, *Fossils and Progress: Paleontology and the Idea of Progressive Evolution in the Nineteenth Century* (New York, Science History Publicat-

ions, 1976); M. J. S. Rudwick, *The Meaning of Fossils* (London, Macdonald, 1972); C. C. Gillispie, *Genesis and Geology* (New York, Harper, 1959); and on the Cambridge reaction, Cannon's works, op. cit. (87).

151. Sedgwick, op. cit. (7), 302-7, on Lyell's *Principles*; Lyell, op. cit. (91), ii, 36.
152. Sedgwick, *ibid.*, 305-6.
153. This in Sedgwick's counter-blast to the Scriptural Cosmogonists in 1830: A. Sedgwick, "Address", *Proc. Geol. Soc.*, 1 (1834), 187-212 (207).
154. Quoted in Bartholomew, op. cit. (57), 281.
155. Lawrence, op. cit. (150), 115.
156. Bartholomew, op. cit. (150), 167. Bartholomew also sees Murchison's *Siluria* as a direct critical response to Lyell, 167-8.
157. Buckland, op. cit. (1), 107, 312.
158. Ibid., 44. He talks of the "chain" being composed of a "uniform and closely connected series" of links.
159. Review of C. Bell, *The Hand, The Lancet*, 1 (1833-4), 165-9 (166).
160. Bartholomew, op. cit. (150), 167.
161. Sedgwick, op. cit. (7), 305.
162. [W. Whewell], "Lyell – Principles of Geology", *British Critic*, 9 (1831), 180-206 (194).
163. Whewell, op. cit. (149), 118.
164. Ibid., 110, 113-6.
165. Buckland, of course, as an expert saurologist, knew all about Geoffroy's teleosaur sequence: op. cit. (1), 252.
166. Clark and Hughes, op. cit. (103), i, 427.
167. Lyell, op. cit. (91), ii, 36; Gillispie, op. cit. (150), 147, on Sedgwick's "phrenesied dream".
168. Geoffroy, op. cit. (112); W. Whewell, *History of the Inductive Sciences* (London, Parker, 1837), iii, 578-60.
169. Morrell and Thackray, op. cit. (89), 252.
170. *The Lancet*, 2 (1830-1), 689.

171. Morrell and Thackray, op. cit. (89), 246-7.
172. Clark and Hughes, op. cit. (103), ii, 26, 144, on his sympathy for Fox's Whiggism; i, 337, 418 on Catholic emancipation and the abolition of tests.
173. Ibid., i, 442.
174. Ibid., ii, 47.
175. R. E. Grant to G. Mantell, 16 July 1850, Alexander Turnbull Library (Wellington, New Zealand), Mantell MS Papers 83, folder 44. Interestingly, Grant did seem to approve of the meetings of German naturalists: "Lectures", *The Lancet*, 1 (1833-4), 93.
176. "Meeting of the British Association in Dublin", *The Lancet*, 2 (1834-5), 695-701 (699). Wakley was being perhaps too optimistic in believing that the BAAS would smash "the disgraceful and impolitic system which has hitherto conferred its ribbons and its garters – its honours and its emoluments, on the most ignorant and worthless portion of society [i.e. the ruling classes]" (688); nothing better highlights the difference between his perceptions of the social role of the society and that of a landed imperialist like Murchison, intent on excluding the lower orders from meetings.
177. P. Egerton, op. cit. (107).
178. F. Egerton, op. cit. (102), xxxv.
179. R. I. Murchison and E. Sabine, "Address", *Report of the British Association for the Advancement of Science* (Glasgow, 1840), xxxv-xlviii (xi-xii).
180. Morrell and Thackray, op. cit. (89), 49:3.
181. R. Owen to Caroline Owen, n.d. [1839], BL Add. MS 39,955, f. 244b.
182. *Report of the British Association for the Advancement of Science* (Plymouth, 1841), xxii.
183. Discussed more fully in Desmond, op. cit. (121); Bowler, op. cit. (150); Gillispie, op. cit. (150).

Chapter 7

The Positive Heuristic Value of Anti-Transmutation: New Directions in Comparative Anatomy and Palaeontology

A Problem of Historiography

Owen's repudiation of Lamarckian-radical thought doubtless endeared him to gentlemen scientists across a wide political spectrum: from old-style Foxite Whigs (Sedgwick) and Peelite Conservatives (Whewell, Buckland) to stauncher Tories hardened against Chartist agitation. But it would be wrong to acquiesce to a century-old tradition and picture his actions as obstructionist and unrewarding; or to accept current positivist critiques of so-called nomothetic creationists like Lyell and Herschel, and accuse Owen likewise of "evading" what with hindsight turns out to have been the "true" problem (i.e. evolution). Such Whiggism may be unfashionable (1), but positivist historiography still remains powerful. Most recently Neal Gillespie in *Charles Darwin and the Problem of Creation* (1979) has asserted that

Herschel's pessimistic qualifications and Lyell's equivocation and the apparent insistence of both on some sort of divine initiative even in a lawful process suggests an evasion of the problem rather than a strong determination to work out a natural solution to it. A truly natural cause of new species could only mean, as Darwin realized, descent, and that could only mean transmutation ... (2).

Gillespie tackles Owen in the 1850s in the same way, strip-

ping his statements from context, and implying that he was dishonestly cloaking his ignorance with words like “creation”. This I believe is a harmful approach; it presupposes a *tension* between supposedly discrete modes of ‘religious’ and ‘scientific’ thought, and uncritically accepts the post-Huxleyan polarization, setting ‘creation’ at odds with ‘evolution’ (3). And it pictures theology as ‘intruding’ into supposedly rational (i.e. reality corresponding) scientific inquiry. As a result Gillespie himself is forced to tear his hero asunder and talk of the “two Darwins”. Rival approaches are less destructive to the actors and more satisfying to historians. J. H. Brooke has taught us to appreciate the finer organic relationship between science and doctrinal theology (4). And Ospovat has suggested that we side-step Huxley’s propagandist distortions by recategorizing historical actors into teleologists and ‘anti’-teleologists (5). This, if not perfect, does at least achieve the result of realigning Owen and Darwin, thus permitting us to reinterpret history from a non-confrontationist perspective.

Even more sensitive works are prone to Whiggish interpretation on the question of species causation. Bartholomew is adamant that Lyell’s talk of the intervention of secondary causes was mere verbiage because he refused to accept transmutation: that is, his “vague statement of faith in the uniformity of nature” was useless for directing him towards “a naturalistic mechanism for species origination”. Transmutation for Bartholomew was the “*only conceivable* model” open

to Lyell, because “there was no conceivable naturalistic means of non-evolutionary species origination that would not have made nonsense of all contemporary beliefs about the laws governing matter” (6). Universal statements imply a huge commitment: the author must be familiar with “all contemporary beliefs” to be sure that no alternative to “evolution” did exist. A Whiggish acceptance of the correctness of evolution and logical assumption that there could have been no natural alternative is no substitute for study of *real* historical knowledge. Bartholomew uncritically, accepts Huxley’s caricature of creationism. What for Huxley was a tactical recasting of history for partisan Darwinian ends has ended up sanctified as official history. An historian whose life’s work has centred on a sympathetic understanding of the ‘Cambridge Network’ might rightly be worried by this uncritical acceptance of polemical history. In 1976 W. Faye Cannon expressed her reservations about “imposing the dogmatisms of modern belief on the earlier period”, suggesting that “it is the emotional acceptance by the modern historian of the ‘rightness’ of evolution which still distorts interpretations of Lyell” (7). Philip Rehbock’s observation that naturalists before the *Origin* saw no necessary connection between law and transformism (8) seems crudely obvious yet is frequently forgotten by historians acting as partisan critics.

So we must be inordinately careful of back-projecting our post-Darwinian views and misinterpreting prominent “legalists

of nature” like Owen (9) who advocated “law” but not transmutatory ascent. Yet criticising Whiggism clearly puts the onus on us to develop viable alternative historical reconstructions. To lay continuing allegations of “equivocation” we must pay strict attention to what anatomists themselves considered important. One option that I have already explored .in *Archetypes and Ancestors* (1982) is to demonstrate by a close study of Owen’s anatomical work in the 1840s-50s that he had a perfectly plausible alternative to transmutation in metagenesis (10). This evidently proved a more satisfying explanation given contemporary needs. (Causes of species origination had to conform to known laws of generation, and be seen in operation today.) This existence of a rival hypothesis should caution us against equating secondary cause with transmutation – as should the glaring fact that this equation was only forged in the middle-class subculture of the 1850s, dominated by X-Club members like T. H. Huxley, John Tyndall, and Herbert Spencer, whose biological determinism, professionalizing need for dissociation from Church control, and allegiance to new bourgeois masters, made it *tactically* important to develop the bond between law and evolution (11)

Recognizing Owen’s rival metagenesis only allows us to make a negative statement – viz. that there was nothing disingenuous in pre-Huxleyan savants accepting law and rejecting transmutation. So here I want to develop the argument in another direction. I will suggest that rather

than Owen's anti-transformist ideology being sterile, it in fact had great heuristic value and led him to newer, more sophisticated approaches to comparative anatomy in 1840s. This is to invert the old positivist tradition. According to that, with anti-evolution quite clearly 'wrong', its defenders could only be obstructionist villains. I believe on the contrary that as a constitutive element in Owen's scientific thought anti-transformism helped generate 'good' science – i.e, new theoretical approaches to comparative anatomy in the 1850s which were considered by contemporaries (and have subsequently been considered by commentators) to have been the most sophisticated 'generalizations' of their age (12). The contrast with Lyell's science is dramatic, and thus my conclusions serve to emphasize another historiographical point. Lawrence's and Bartholomew's conclusion that Lyell's non-progressionist strategy was largely unsuccessful could be used to reinforce the older sociological canons that 'ideology' was solely a *distorting* factor. It could be used to justify Joseph Ben-David's claim that the only workable sociology of knowledge concerned itself with bias (13). This, in effect, would return us to our positivist starting point where 'religion', which fostered a pernicious anti-Lamarckism, was presumed 'subversive' to the true course of 'rational' inquiry. Owen's anti-transformist strategy, by contrast, was essential to the generation of culturally accredited science: his ideology cannot be dismissed as a 'distorting' factor.

Owen's Hunterian Lectures of 1837

and his Use of Von Baerian Embryology

The Progress of knowledge will, of course, be impeded in proportion to the influence and popularity of those Teachers who for the sake of the small and transient reputation, gained by exciting the wonder of their hearers, or readers, advocate the baseless speculations of the transcendentalists, and indulge in exaggerated expressions of views to the development of which they are unable or unwilling to lend the co-operation of honest and unbiased labours.

Owen speaking at the end of his fourth Hunterian Lecture,
9 May 1837 (14).

A characteristic of Owen's Hunterian Lectures in the late 1830s and early 1840s was their denial of recapitulation and advocacy of Karl Ernst von Baer's embryology of divergence. Dov Ospovat has greatly extended our knowledge of Owen's use of von Baerian foetal development "as the analogical basis for a new palaeontological theory" (15), which Owen publicly announced in 1851 (16). Von Baer himself had rejected recapitulation, not so much in immediate consequence of his studies on chick development, but because of his *a priori* antagonism to unilinear conceptions of nature (17). (Recapitulation during ontogeny of the adult forms of lower animals necessarily demanded a hierarchical series: thus recapitulation was logically related to lineal progression.) According to Ospovat, Owen was stimulated by Martin Barry's articles on embryogeny and 'unity of structure' which appeared in Jameson's Journal in 1836-7 (18). Certainly

Carpenter, who as Grant's student had already been introduced to Geoffroy's extended 'unity' doctrine, was ready to accept Barry's conclusions when they were published in 1836. On reading "Dr Barry's valuable papers" Carpenter – then at Edinburgh Medical School – penned his own essay for Jameson's Journal on the "Unity of Function in Organized Beings" (19).

Since Carpenter was stimulated to read von Baer after Barry's prodding, I see no reason to doubt Ospovat's belief that Owen was prompted likewise. However I think that we can take the issue a stage further, and ask why Owen was so *receptive* to von Baer's embryology (a separate issue from discovering who directed him to von Baer's works). The importance of this question becomes apparent after reading the MSS of Owen's first series of Hunterian Lectures (1837) – housed partly in the British Museum (Natural History) and partly in the Royal College of Surgeons. In these Owen persistently links the issues of transcendental anatomy, embryology, and anti-transmutation; moreover, his emphasis leaves no doubt that there was a strategic anti-Lamarckian payoff in his moving to von Baerian divergence.

To understand the context of Owen's shifting allegiance, it would be well to recap the salient features of Grantian development. Grant remember modelled his science on Geoffroy's and Blainville's. He rested his transformist case on three interrelated factors: a lineal progression of life from monad to man, a "unity of composition" holding

throughout the entire series, and a confirmatory recapitulation of the sequence during foetal development. For a time Blainville accepted all three propositions; and although he denied any transformist implications, he did have ‘political’ anti-Cuvierian reasons for championing a Lamarckian chain (20). And from their sharing certain fundamental assumptions, Blainville and Grant ended up as allies determined to stop anachronistic fossils like the Stonesfield opossum from destroying the serial progression. All three anatomists, as a result of their belief in lineal progression, played down or frankly emasculated Cuvier’s discrete *embranchements*. They did this by seeking a continuous series of forms spanning the *embranchements*. Blainville established homological relations between “internal Osteozoa” (i.e. vertebrates) and “external Osteozoa” (Articulates) as early as 1816 (21). And following Geoffroy’s celebrated clash with Cuvier at the Académie over the analogical relationship between cephalopods and fishes, Grant similarly established a continuum between molluscs and lowly vertebrates. All three advocated the identity of piscine opercular plates, elements of the reptilian lower jaw, and mammalian inner ear ossicles (22). So before 1839 – when Blainville renounced the ‘unity’ principle and a staggered progression in favour of a single creation – Grant’s views were typical of French thought, and of Geoffroyan transformism in particular. In 1830 we find him already applying universal taxonomic criteria which would

allow a serial ranking of organisms and the application of the doctrine of “unity of composition” to the entire series.

Common assumptions about the unity of life and the animal series extended to recapitulationist embryology – Grant, Knox, Geoffroy, and Blainville were all recapitulationists (23). Grant employed flagrantly recapitulationist language in his LU and Royal Institution lectures. Talking of the nervous system at the RI in 1834, he is reported to have “amused his audience, when he came to the human being, by informing them that at one period of foetal life, the brain of the future man is that of a tadpole, at another that of a fish, and subsequently that of a crocodile” (24). Greater novelty attached to his views of the ontogenetic development of the liver (25). In 1837, shortly before taking the Fullerian Chair, he described to his Albemarle Street auditors the development of the liver in the animal series – follicular in infusoria, glandular and opening directly into the digestive sac in the “lower animals”, but connected by a duct in higher ones. He then likened this to the stages of liver development in the human embryo. Here “the first appearance of any thing indicative of the future liver is a mere thickening of the intestine at a particular spot; that thickening then becomes more and more defined, is found to be follicular, glandular, lobular, and at length apart from the alimentary canal, to which it is connected merely by a duct” (26). This recapitulation from monad to man supported Grant’s faith in a transcendental ‘unity’ throughout the animal kingdom.

Understanding this context of transmutation in 1837 puts us in a better position to appreciate Owen's strategy in the Hunterian lectures. I now want to turn to an analysis of this, to reveal how he used von Baer's law to curb this application of Geoffroy's 'unity', deliberately destroying the foundation of the transformists' case. His *ex cathedra* pronouncements on the impossibility of transmutation in his lecture on the "Nature and Characters of Organized Beings" (11 May 1837) leave no doubt of his prior commitment to refuting transmutation. Here he discusses the "organizing energy" which regulates structural growth to produce a well adapted form (27). This "organizing principle" distinguishes crystal growth from organic development. It operates, he wrote, "according to laws of Intelligence and Design; but we must not fall into the error of assigning to it the attributes of a conscious soul" (28). It was an "unconscious power" and like other "imponderable agents" worked "according to determinate laws, which manifest in the highest degree the wisdom and design of the Lawgiver" (29). Organic mutation in response to changing conditions was precluded by the invariant, determinate action of this "unconscious power", which always functioned to build the same organic mechanism. Thus "Each species of Organism is self-existent from the period of its Creation" and "maintains its specific character unchanged", finally disappearing "with the extermination of the reproductive Individuals; for the Genus has no power to

reproduce the Species, nor the Family the Genus". So Owen began with premises which undermined not only Grant's materialist transformism, but left him unable to encompass Chambers' providential generation seven years later. By his canons, immutable laws made immutable species; the "organizing energy" was limited and could not be spontaneously stretched to produce new organs, which would be tantamount to investing the organism with "self-developing energies" (30).

The individuals of each species have a characteristic durability of Life; - the operation of the Organizing energy in them is limited. To suppose a power of prolonging the vital actions in an individual beyond the specific period, is to suppose that the organizing agent has the power to develope new organs, or to modify the old ~~to such an extent~~ as must cause a transmutation of the Species: – but this supposition cannot be maintained (31).

Extinction, not mutation, greeted organisms at the onset of new environmental conditions. Moreover, study of extinct species confirms that there was no necessary ascent (32) of the sort demanded by transformists. Nor have the genetic descendants of long-dead individuals noticeably changed, as is evidenced by the mummified ibis brought back from Egypt after Napoleon's campaign.

So the first point about the lectures is their *overt* anti-transformism. The second concerns his attempt to limit Geoffroy's 'unity' and thus restrict its usefulness to transformists like Grant. He made it clear that he was worried by irresponsible applications of Geoffroy's doctrine. Following

the success of the theory of analogies for vertebrate classes

some Anatomists, and especially those whose knowledge happened to be limited to a single great Division of the Animal Kingdom, were led to form Theories which are undoubtedly new, as regards their extravagance: assuming in these Speculations that Nature is restricted in the development of Animals to a supposed Unity of Composition; – a unity of Plan; – and a constancy of Connections; [inserted: & also a certain number and kind of component parts, which are all determined by an *a priori* theory] they have proposed most extraordinary Analogies (33).

One such was Geoffroy's identification of the mammalian inner ear ossicle with the opercular bones, which Owen countered by invoking a rival teleological explanation: the bones of the gill cover in his view were by contrast *adaptive*, i.e. "subservient to the mode of Respiration peculiar to that Class". We have seen that he periodically invoked *functional* explanations when it was ideologically convenient – e.g. he scotched the idea of the relations mooted by Grant between low fish (the sturgeon) and high invertebrates by insisting that the sturgeon's "scale armour" was not a remnant of invertebrate shell but ballast necessitated by the fish's bottom-grubbing existence (34). Thus he suffered no qualms about endorsing teleological explanations when needs arose. Although when there was no immediate point to be served he superimposed teleological adaptation on the common plan, i.e. made teleology subservient to homology – for which reason Ospovat treats him as an *anti*-teleologist (35). The *context* so often dictated Owen's choice of approach; he *used* teleology against the transmutationists, even if this was not

his primary mode of thought. Again, in his criticism of the opercular-ossicle analogy, he was attempting to defuse Geoffroy's principle and make it inoperative in a transformist sense. This and "many similar" strained relationships adopted by Geoffroy and his 'followers' (Knox and Grant in Britain), Owen concluded, "are the result of an abuse of a sound and fruitful Principle, which has only suffered by an unwarrantable extent, and unjustifiable mode of its application" (36).

In the same way, he finished his fourth lecture on 9 May with a review of the science since Hunter's time which turned into an ill-concealed attack on transcendental excesses. Besides ridiculing those who postulated a single vertebral element "in every bone of the vertebrate body ... in each ring of the Worm, and in every joint of the Lobster", he also criticized *a priori* determinations of cranial homologies, the 7-vertebra theory of the skull, the 9-element vertebral theory, and so on (37). He is not disputing the *principle*: that the skull comprised modified vertebrae, or that vertebrae were composed of standard components, so much as a dogmatic adherence to specific numbers* It might seem

* Recall that Grant had been instilling Geoffroyan ideas of skull and vertebral composition into his students at the university since its foundation. He also explained Geoffroy's theories at the Royal Institution. In June 1834, his *soirée* lecture concerned the development of the vertebral column, and consisted of an "explanation of M. Geoffroy de St. Hilaire's method of contemplating the bony structure of animals" (38). Grant adopted a 7-element vertebra. In this lecture he went on to discuss Geoffroy's theory of the

odd that he should do this given his later work on archetypes and homologies, in which, as is well known, he defended the notion of cranial vertebrae. The explanation seems simple at first sight, since it was the *transformists* who opted for a 7-vertebra skull. But Owen's attack was much more subtle. He implied that such "arbitrary" figures were suspect because they were not based on embryonal researches – and he did this because he wanted to endorse a particular sort of embryology for fundamental ideological reasons. He insisted that such *a priori* assumptions are "wholly unsupported by an examination into the primary formation of the cranial bones", which alone allow us "to determine how many [cranial vertebrae] are actually developed from the circumference of agglutinous *Chorda dorsalis*; the only "true embryological condition of a Vertebrae" (39). Failure to appreciate the importance of embryology even led Cuvier astray:

It was to obviate the retrograde tendencies of these Metaphysical or transcendental Theories of Animal Organization that Cuvier devoted his latest energies; and the abuse of the doctrine of Analogies perhaps led him by a natural reaction to underrate its value.

The general laws of Animal Organization can never be developed from a consideration of the perfect or matured structure alone: and such *was* the general character of the knowledge from which Cuvier deduced his inferences.

The laws of Coexistence; – the adaptation of structure to function; and to a certain extent the elucidation of natural affinities may be legitimately founded upon the examination of fully developed species: – But to obtain an insight into the

vertebral-skull; and in *Outlines of Comparative Anatomy* (1835) gave his assent to Geoffroy's 7-vertebra theory (a fact Owen duly noted).

laws of development, – the signification or bedeutung of the parts of an animal body demands a patient examination of the successive stages of their development, in every group of Animals (40).

This brings me to the gist of the argument, which is to demonstrate the direct benefit to Owen qua anti-transformist of von Baerian embryogeny – and to the ideological payoff of insisting that only the “truly philosophic inquiry” of von Baer and the German embryologists could lay a permanent foundation for “a just and true theory of animal development and organic affinities” (41). Von Baerian embryonic divergence helped in two ways. First, it directly broke the recapitulatory crutch supporting serial transmutation, and second, it more subtly destroyed the possibility of homological (and therefore transmutational) relations between members of different *embranchements*. To take this second case first: Ospovat rightly suggests (42) that in Owen’s *Lectures on the Comparative Anatomy of the Invertebrate Animals* (the 1843 Hunterian Course), von Baerian specialisation was extremely useful to him in setting strict limits to Geoffroy’s ‘unity’. Owen ostensibly set out to test those limits, as he said on introducing his conclusions at the end of the *Lectures*: “we may now attempt a more exact enunciation of the resemblance which a higher organised animal presents to those of a lower order in its progress to maturity; and the consequent extent to which the law of ‘Unity of Composition’ may be justly, and without perversion of terms, be predicated of animal structures” (43). He proceeded to demonstrate that, rather than recapitulate the entire

inferior series, each embryo according to von Baer developed from a germ towards the characteristic organization of its *embranchement*. Embryonic divergence thus resulted in the typical archetype for that *embranchement*. Such specialisation meant that a higher animal during ontogeny “does not represent all the inferior forms, nor acquire the organization of any of the forms which it transitorily represents” (44). No unilinear series existed, and therefore the search for taxonomic criteria by Grant and Geoffroy which would allow us to define a ‘Unity’ from monad to man was a quest for a transformist holy grail. Owen rammed home this point:

Had the animal kingdom formed, as was once supposed, a single and continuous chain of being progressively ascending from the Monad to the Man, unity of Organisation might then have been demonstrated to the extent in which the theory has been maintained by the disciples of the Geoffroyan school (45).

The transformist implication (which Ospovat does not mention) is this: because structural unity between, say, cephalopods and fishes, was restricted to the primary or germ stage of foetal development – after which there was a fundamental divergence – it was impossible to conceive of a squid turning vertebrate by mutating into a fish. By *using* embryology to split life into discrete groups, each with its irreducible plan, Owen destroyed the transformists’ continuum and dashed hopes for a wider unity.

But I said that von Baer was more directly useful, in breaking the recapitulatory support of transmutation, and here we have to return to the 1837 MS. Owen's testimony at the end of the fourth lecture reinforces the conclusion that he found von Baer essential for ideological reasons. The sequence of topics introduced on 9 May was itself telling. He first spoke of the new embryology showing us the true path. In the next sentence derided "those Teachers" who "advocate the baseless speculations of the transcendentalists". And as if to confirm that he had in mind the likes of Grant (46) and Geoffroy, he then complained

It is thus that the beautiful observation of the resemblance of the imperfect condition of the organs of a higher species to the perfect conditions of corresponding organs in a lower organized species is misrepresented when it is stated that the Human Embryo *repeats in its development* the structure of any part of another animal; or that it *passes through the forms* of the lower classes; or when it is asserted that a Fish is an overgrown Tadpole. Such propositions you will at once ... perceive, imply that there exists in the Animal Sphere a scale of Structure differing *in degree* alone: – nay, they imply the possibility of an individual, at certain periods of its development, laying down its individuality, and assuming that of another Animal; – which would, in fact abolish its existence as a determinate concrete reality (47).

So there was no doubt that Owen *interpreted* recapitulation primarily in a transformist sense; by destroying one with von Baer's doctrine, he believed he could destroy the other. Recapitulation for him consisted of a literal mutation during embryogeny, which repeated in microcosm the alleged metamorphoses of fossil ascent. Both obliterated individual existence; as such they raised immense theological problems,

since morphological differences between man and beast became one of degree.

Owen reassured his respectable audience that “Individualities, however, manifest themselves at very early periods of development [in von Baer’s scheme], and cannot be laid aside” (48). The twin manifestations of transformism were thus indissolubly linked in Owen’s mind, as he made plain when he noted in conclusion:

The doctrine of Transmutation of forms during the Embryonal phases, is closely allied to that still more objectionable one, the transmutation of Species. Both propositions are crushed in an instant when disrobed of the figurative expressions in which they are often enveloped; and examined by the light of a severe logic.

Owen was probably not the first to attribute to transformists the belief that a direct relationship existed between foetal and fossil development and that both involved a mutation and obliteration of individuality. Nor was he alone in rejecting recapitulation for that reason. Jane Oppenheimer suggests that von Baer himself might actually have employed divergence to scotch theories of the mutation of lower into higher organisms (49). But Owen’s position was more consistent and thorough than that of contemporaries like Lyell. This is because Lyell (being no anatomist) was in the tactically different position of having to *accept* Etienne Serres’ empirical evidence while trying to disavow Lamarckism. Unlike Owen, he could not *afford* to assume the identity of transformism and recapitulation. He would only state that recapitulation “has appeared to some persons to

afford a distant analogy, at least, to that progressive development by which some of the inferior species may have gradually perfected into those of more complex organization" (50). Serres' researches had admittedly shown

that in the passage from the embryo to the perfect mammifer, there is a typical representation, as it were, of all those transformations which the primitive species are supposed to have undergone during a long series of generations, between the present period and the remotest geological era.

Teratological researches may also succeed in "freezing" the embryo at some lower stage. (This, as Lyell knew, was what Geoffroy was attempting with his chick experiments.) But this would only demonstrate

the unity of plan that runs through the organization of the whole series of vertebrated animals; [it lends] no support whatever to the notion of a gradual transmutation of one species into another, least of all of the passage, in the course of many generations, from an animal of a more simple, to one of a more complex structure (51).

It was because Owen now *rejected* what Lyell had been forced to accept, i.e. linear recapitulation, that it suddenly became tactically useful to insist that fossil and foetal development were inextricably linked. By refuting Serres' recapitulation, the transmutation theory would be seen to lose its strongest scientific support.

In summary: Owen's abhorrence of transmutation in its brutalizing aspect – in its ability to obliterate individual existence and make man morphologically indistinct from the

beasts – made von Baer’s doctrine immensely attractive. With it he could set strict limits to Geoffroy’s ‘unity’, relegating the common morphological traits shared by members of discrete *embranchements* to the germ stage. Dispensing with the organic continuum, he effectively dashed transformists’ hopes of bridging the gaps between divisions, e.g. molluscs and vertebrates. Without doubt we can talk of the positive heuristic value of his anti-transformist ideology. As a guiding principle it made the new German embryology profitable in more than an esoteric, scientific sense. It became a powerful tool in his social struggle against a scientific rival in London like Grant, at a time of political crisis when the social elite was desperately concerned to make scientific theory morally impregnable and serviceable to the masters of social order. The scientific implications of Owen’s move are in fact not exhausted yet. For having accepted the divergence model, Owen took the innovative step of applying it to the fossil record to generate an image of progressive specialisation to rival the serialists’ simplistic ascent. In this way he could explain successive development without invoking any sort of necessary fossil progress. Thus he pioneered what Ospovat lavishly calls “the most modern and sophisticated version of progressionism” and Bowler a “revolutionary” development in palaeontology (52). Since it was onto this divergence model that Darwinism was to be mapped, Owen’s anti-transformist strategy had the indirect and ironic effect of supporting the subsequent evolutionary superstructure. The relationship of

Owen's views on "Progress" in post-*Origin* times to those of Darwin, Huxley, and Spencer is a subject of labyrinthine complexity; and it is not the place to go into it here (the issue is discussed at length in my *Archetypes and Ancestors*). But even looking at the larger pre-Darwinian picture, it is clearly incorrect to dismiss Owen as an obstructionist (which is how he is often portrayed), when his Peelite approach to cautious reform in morphology and palaeontology ushered in a sophisticated morphological world view and arguably hastened the acceptance of Darwinism.

Owen, Buckland, and a Palaeontological Consequence of the Blainville-Grant Model of Serial Ascent The Case of the Stonesfield 'Opossum'

The final judgment of M. de Blainville met with approbation and support from the stricter systematists, since it harmonized with their preconceived opinions on the progressive appearance of organized forms on this planet.

Owen explaining the opposition of Grant and Blainville to the diagnosis of the Stonesfield jaws as mammalian (53).

Owen not only tackled Grant on the higher theoretical aspects of transcendentalism, but on the specific zoological 'deductions' of his serial transformist doctrine. The most important of these concerned the interpretation of the famous Stonesfield 'opossum' jaws. The evidence I shall now present shows how Owen challenged his Gower Street rival on this subject at the GS in 1838-9, egged on by his Oxbridge admirers. My documents only allow me to follow up the support

shown by William Buckland; however, knowing the social composition of the Somerset House Society, and the hierarchy of Oxbridge dons (Sedgwick, Whewell, and Buckland) and gentlemen of substance from Lyell to landed Tories like Murchison, Sir Philip Egerton and the Earl of Enniskillen, I suspect that support for Owen on the occasions of his clashes with Grant was strong. Certainly he was well favoured by the Society. He was elected to the Council in 1838, and was “wonderfully pleased” to receive the Wollaston Medal earlier that year for his work on Darwin’s South American *Toxodon* (54)). His school fighting chum Whewell was in the Chair that year and at the presentation made “a very handsome speech”, Owen told his sister, “alluding to me very feelingly as a fellow-townsman and old schoolfellow” (55). But Buckland was a closer personal acquaintance, the families having grown close after the Owens honeymooned in Oxford in 1835 (56). Buckland had attended Owen’s 1837 Hunterian lectures, with Mrs Buckland taking the opportunity to visit Caroline Owen. In September 1838 the Bucklands and Owens were in Freiburg together as guests of the association of German naturalists. Their letters at this time show how broadly their interests overlapped, from agreeing the function of *Nautilus*’ siphuncle (of great excitement because Blainville was working on the problem (57)) to collaborating on the Oxford saurians. Buckland supplied Owen with plesiosaur paddles and the like from his collection, preparatory to Owen’s study for the BAAS. (Buckland actually carried fossils with him to London,

or had Lord Cole deliver them to Lincoln's Inn (58).) He made a note of being present at the reading of Owen's papers, and advised him on the means of obtaining BAAS grants, and a publisher who could offer the best terms for his work on odontography (59). For his part Owen actively courted the patronage of the Oxford divine and made good use of his Bridgewater Treatise in his own Hunterian lectures.

Buckland and Owen agreed in their interpretation of the fossil record, and on the need to establish a punctuated progression in order to put an end to transformist demands for inexorable ascent. In pursuance of this goal, they took steps against zoologists who refused to accept a mammalian diagnosis of the Stonesfield (Jurassic) jaws, targeting specifically Grant, who persisted in arguing for their reptilian nature. (By serialist criteria, a mammalian diagnosis would have made them fossil anachronisms – mammals out of sequence, living in the Secondary age when deoxygenated palaeoclimatic conditions permitted only a reptilian grade of development.) So in this section we will be concentrating on the Oxford fossils to highlight the degree to which interpretations can be coloured by prior theoretical considerations. Not that all morphologists voted along ‘party’ lines – Geoffroy on viewing Buckland’s jaws in Paris in 1838 conceded their mammalian nature (60). A block vote would anyway have been surprising given the complexity of the argument, the number of theoretical issues, and the national and cultural diversity of the protagonists (Owen,

Buckland, Grant, and Ogilby were British; Valencienne and Blainville French, Agassiz Swiss, and Harlan American). Since cultural expectations varied with individual cases, it will be necessary to restrict our discussion to a specific locus, the GS; this is crucial if we are to follow up *personal* interactions and offer a social explanation. (It also happens that the major events took place here, in 1838-9, and intimately involved Grant.)

Until the 1830s controversy centred on the jaws' antiquity, not their diagnosis. Coming from the Oxford Stonesfield Slate, they had first fallen into possession of the Oxonians. Broderip, as a wealthy undergraduate in 1812, had acquired the first two jaws (61), which had been discovered by an old stone-mason who regularly collected for him (62). Both were in matrix, and Buckland persuaded Broderip to part with one for a price. The other he apparently lent Buckland for examination. Both master and pupil believed the jaws to be mammalian but because of their exceptional antiquity refrained from publishing until Cuvier had had a chance to examine them. Visiting Oxford in 1818 Cuvier compared the jaws to those of the opossum *Didelphis*. Still Buckland did not announce their existence until 1824, when he mentioned them in his *Megalosaurus* paper (63). The following year Constant Prévost (1787-1856), refusing to credit that a mammal could be interred in rocks so ancient, reinterpreted the Stonesfield Slate as a post-chalk deposit

(64). (Interestingly Prévost was to become a Blainvillean serialist.) Cuvier in his *Ossemens Fossiles* confirmed from Prévost's drawing that Buckland's animal was an opossum-like "Carnassial", but with a longer tooth-row (Buckland's jaw had ten grinders). He admitted that "if this animal be really from the schist of Stonesfield, it is a most remarkable exception to an otherwise very good rule, that the strata of that high antiquity do not contain the remains of mammals" (65). Broderip's jaw meanwhile had been mislaid and only recovered in 1827, when he published a description in the *Zoological Journal*, pointing out that, with only seven grinders, this fossil was "generically different" from Buckland's, and closer in dental formula to an opossum. For "the sake of convenience" he designated it *Didelphis Bucklandii* (66) to distinguish it from Buckland's specimen which Cuvier had christened *D. Prevostii*. (He also noted that these didelphids were part of a fossil fauna, including *Trigonia*, much like that surviving in the Australian colonies; this seemed additional proof of their marsupial nature.) By the 1830s all major English geologists, including Lyell, Sedgwick, Buckland, Fitton, Mantell, and Phillips (67) accepted Cuvier's diagnosis and an Oolitic (Jurassic) age for the embedding Stonesfield slate. And some at least (Lyell, though not Buckland at this stage (68)), already recognized their value in anti-transformist terms.

Believing that the same simple laws regulated fossil and foetal development, Grant projected the Blainville-Serres

serial model onto the palaeontological record to generate an image of *linear* fossil development. This was confirmed by the empirical finding that reptiles flourished in Wealden and Cretaceous times, and were superseded by mammals in the Eocene. Environmental theories to account for this were well advanced at the time. Even Buckland, with his Paleyite-Bridgewater approach and determination to see each animal's "cluster of contrivances" lock it into a specific econiche, was inconvenienced by an odd Oolitic mammal. He observed in *Geology and Mineralogy* (1837) that global conditions, judging by the Secondary fauna, "seem not to have been sufficiently advanced in tranquillity, to admit of general occupation by warm-blooded terrestrial Mammalia" (69). On the other hand, Owen was shortly to speculate on the well-developed respiratory apparatus of dinosaurs, suggesting advanced environmental conditions in later Secondary times (70), so he could probably more easily accommodate to the idea of Jurassic mammals. Grant himself probably drew support from Geoffroy's fourth memoir on teleosaurs (1833), where Geoffroy had argued that the birth of mammals and birds in the age of saurians was unlikely on account of their unsuitable respiratory mechanics (71). Anyway, Grant was first, so far as I can tell (72), to dissent from the opossum diagnosis on purely *diagnostic* grounds. In 1834 he acknowledged that Cuvier's Montmartre excavations had revealed opossums in the Eocene, but asserted that the more ancient Stonesfield jaws had been "erroneously ascribed to the same animal" (73).

Thus began a period of growing dissension over the diagnosis of the jaws which culminated in the clashes at the GS in January 1839. A number of other historians have studied this episode. Peter Bowler in *Fossils and Progress* notices that the fossils upset Cuvier's step-wise progression, yet treats the episode inductively, with Owen finally triumphing in his correct interpretation (74). Patsy Gerstner has commented on the Transatlantic connection, since the Philadelphian Richard Harlan (1796-1843) became embroiled because of his reptilian diagnosis of *Basilosaurus* and appearance at the GS at the crucial time (see below). Gerstner's Transatlantic approach is useful, but I believe that Owen's 'co-operation' with Harlan was far from an attempt to further Anglo-American relations; it was a deliberate part of his strategy for weakening the palaeontological base of serial transmutation. Neither Bowler nor Gerstner give any indication of having read the relevant MS letters. Nor have they noticed Grant's role in the Stonesfield affair, and by cutting out the London *transmutationist* they have missed a critical aspect of the debate. This is odd, since Owen himself in his *published* works after 1839 (when Grant reasserted his position in his *General View*) makes it plain that the Gower Street professor was a leading protagonist (75), and the letters from Buckland confirm that their strategy was pre-eminently aimed at combatting his dissenting view. This of course throws a new light on the affair. The episode can now be painted on to the larger

social and political canvass prepared in the previous chapters. It was part of the social response; Owen, Buckland, and the gentlemen of the GS were intent on highlighting the absurdity of inexorable ascent and change, of a system divorced from the moral influence of theology and self-sustaining under its own Lamarckian energies. Neutral, apolitical geological inquiry (the propagandist image of Anglican science) was to be presented as final arbiter of the fate of radical transformism.

Grant disputed the ‘opossum’ diagnosis at least a year before Agassiz (in 1835) began to express his own doubts. (There is some confusion over whether Agassiz knew of Grant’s more extreme opposition (76).) Grant’s dissension was well known in Paris by 1838; M. de Roissy pointed out Grant’s views to his friend Blainville (77), who referred to Grant constantly when voicing his own doubts in 1838 and 1839 (78). By this time Grant must have been going into detail in his lectures, for Blainville wrote in 1839 that he (Grant) “had advanced the same opinion [viz. that the jaws did not belong to any mammal] in his course of Lectures this year, at the same time assigning his reasons for it” (79). Grant himself was less equivocal than Blainville. The latter in fact conceded that Buckland’s jaw (*D. Prevostii*) was like that of a tree shrew, even though the teeth differed in number, place, and shape. And Broderip’s jaw (*D. Bucklandii*) really *was* similar to that of the Virginian opossum. It possessed an

identical number of incisors, canines, and molars (even though they differed in shape, being more like those of *D. Prevostii*). The similarities suggested that the two fossils belonged to the same class. But which one? At one point Blainville came close to conceding that they might really be mammalian. Only mammals had simple crowns and roots in the anterior teeth and complex crowns and roots in the molars (80). On the strength of it being possibly a non-didelphid mammal, he coined the non-committal name *Amphitherium*. Then he unexpectedly recalled the tooth structure of Harlan's fossil saurian *Basilosaurus*, which also possessed molars with complex crowns and roots. This caused him to equivocate and finally change his mind, for he finished:

we can not hesitate to admit that if this *Basilosaurus* be a reptile, which fact appears placed beyond doubt by the form of the *vertebrae, humerus, &c.*, it is more than probable that it is an animal of the same kind as that found at Stonesfield (81).

If at length he was cautious, it was because he had not personally examined the fossils. In his "Nouveaux Doutes" (1838), he retained the name *Amphitherium* although still considering the animals as most likely oviparous, a diagnosis more in accord with "the age and the geological characters of the formation which contains the fossils in question, as well as with the organized bodies with which they are associated" (82).

Grant consistently denied the opossum diagnosis, and in the *General View* (1839) three times noted this incorrect

identification of *Amphitherium* (he immediately accepted Blainville's name). He argued that "no unequivocal skeleton of bird or quadruped has occurred in [the newer oolitic strata], although the supposed mammiferous jaws of *amphitherium* have been found in the oolitic slate of Stonesfield which have been erroneously referred to the marsupial genus *didelphis* ... so that the authenticated extinct species of birds and mammalia appear as yet to be limited to the tertiary strata" (83). The oldest opossum was still Cuvier's from the Tertiary gypsum of Montmartre. Grant by now had examined the four known Jurassic jaws, as well as the fifth indisputably reptilian one from the same strata preserved in Paris. He gave a detailed account of the comparative relations of the jaws and teeth, claimed to have identified the jaws' composite structure, and agreed that Harlan's American *Basilosaurus* was a "closely allied" genus (84).

A Related Problem: the Nature of *Cheirotherium* Tracks

I do not want to give the impression that Grant's serialist predisposition was by its nature destructive to good science. Frequently it was indispensable to him in ridding palaeontology of even uglier anachronisms. Hence in the *General View* he raised another point, that of the correct interpretation of the *Cheirotherium* tracks in New Red Sandstone (85) (another issue that was to come up in the decisive December 1838 January 1839 meetings of the GS). Footprints

found in Secondary rocks in Saxony had been referred to marsupials by Johann Kaup (1803-1873). Buckland introduced Kaup's views to the English in *Geology and Mineralogy*; and although he himself had interpreted tracks of similar age in Dumfries as belonging to tortoises, he nonetheless gives Kaup's views credence

Professor Kaup has proposed the provisional name of Chirotherium for the great unknown animal ... and he conjectures that they may have been derived from some quadruped allied to the Marsupialia. The presence of two small fossil mammalia related to the Opossum, in the Oolitic formation of Stonesfield ... are circumstances which give probability to such a conjecture. In the Kangaroo, the first toe of the fore-foot is set obliquely to the others, like a thumb, and the disproportion between the fore and hind foot is also very great [Kaup's prints were hand-like, and the hind feet twice the size of the forefeet] (86).

Triassic kangaroos were, if not fatal to Grant's ultra serialism, then at least cause for concern. Thus when he went to Liverpool to lecture at the Mechanics' Institute in August 1838, he made a point of examining large *Cheirotherium* tracks like Kaup's which had been discovered in June 1837 (87) in Stourton Hill Quarry five miles from the city centre. Grant's lecture was reported in the *Liverpool Mercury*, and extracted almost entire (with only his opening remarks on the undeviating progression of life excluded) in the *Magazine of Natural History*, which had already run translations of Blainville's *Amphitherium* papers. The editor's introductory remarks make it plain that the two issues were practically conflated in the public mind:

The determination of the zoological relations of the fossil jaws from the Stonesfield strata, and also those of the animals whose footmarks have been left in rocks of still higher antiquity, are two subjects now before the scientific world With respect to the sandstone impressions, this subject is so nearly related to that of the “supposed fossil didelphs,” that we are induced (knowing the report to be a correct one) to quote the following article ... (88).

In the lecture Grant urged caution in interpreting the sandstone prints, and pointed out the improbability of their having been made by a member of the “elevated” classes.

In a letter to Jameson’s journal he had already dismissed General Lord Greenock’s “wolf’s” tooth as that of a fish like *Lepisosteus* (89); now to illustrate the fate awaiting all such ludicrous anachronisms, he listed a catalogue of errors from Scheuchzer’s *Homo diluvii testis* to the Dumfries tortoise tracks mistaken for those of a dog or human. The free thumb on the Stourton prints, he said, could as easily be attributed to an ape as a kangaroo, and the only reason geologists opted for marsupials was “from a belief that certain bones found in the oolites of Stonesfield, have been determined to belong to *Mammalia* of this order” (90). He then described to the mechanics how such impressions were formed, and showed them four hindfoot prints, about nine inches long. These, he said, were always preceded by smaller forefoot prints, four inches long, with a free thumb. He then cleverly reinterpreted the tracks, fitting them into a normal crocodile sequence (91). He switched the right foot for the left, so instead of a prehensile *thumb* (on, say, the right foot), this became the rudimentary outer toe (on the left).

No longer would the animal have some peculiar marsupial gait, a mammalian duck-walk, crossing its own line of gravity at each step. (Others too concluded that its right and left feet must have overlapped by three inches when walking (92).) Grant not only rendered the prints wholly unexceptionable, they were exactly what we should have expected, had they been made by the teleosaurs common in the period.

He went on to list supporting facts. He noted smooth depressions in the sandstone as if a reptile “between the efforts of progression, had rested its belly, on the ground”. The claw marks were crocodilian; the deep heel marks suggested “heavy-bodied and feeble-footed reptiles”. Despite the uncertainty in dealing with tracks from such antiquity, he could reasonably conclude that these tracks were not proof of “the existence of hot-blooded mammiferous quadrupeds at the remote period assigned to the deposition of this new red sandstone” (93).

Grant’s reconstruction was convincing. The trouble was, the issue had become so entangled with that of the Stonesfield jaw from the younger Oolitic deposits, that any attack on the “marsupial” tracks could have been seen as weakening the evidence for a marsupial diagnosis of the jaws (particularly since Buckland had linked the two). This would have been especially worrying by late 1838, when Blainville came forward to support Grant on the Stonesfield question.

The Debate over the Stonesfield Jaws

By now the jaws had acquired an international “notoriety” (94), but their continuing prestige rested solely on their being truly mammalian. As *saurian* jaws, these Jurassic fossils would have lost all distinction. With disaffection spreading to Paris and Germany, Buckland now undertook a European tour, carrying with him two jaws (a *D. Prevostii* and *D. Bucklandii*). Just missing Blainville in Paris (who had departed for the country (95)), he left the jaws with the descriptive zoologist Achille Valenciennes (1794-1865) for casting (copies being presented to members of the Académie). Buckland’s gamble paid off: a flurry of papers appeared in the *Comptes Rendus*. Valencienne predictably vindicated his former patron Cuvier. Notwithstanding assertions to the contrary (originating, he thought, with Grant), Valenciennes concluded that each jaw was composed of a single dentary, making it indisputably mammalian (*saurian* jaws were composite). What allowed Valenciennes to be so positive was his use, not of the common Virginian opossum for comparison, but the small *D. murina* which was much closer to the fossil jaws in structure. He admitted that the fossils represented a distinct genus of didelph-like marsupial, and finding nothing ambiguous about them, he proposed substituting the name *Thylacotherium* for Blainville’s *Amphitherium** This must have

* By the end of the year such a plethora of names were in use that it was confusing for all concerned. The situation was made worse by Owen at first accepting the name *Thylacotherium*

been gratifying to Buckland, though better was to come. In the *Comptes Rendus* Geoffroy himself signalled his conversion. Finally, in September 1838, Buckland took the fossils on to Freiburg (where he joined up with Owen) to place the problem – and the jaws – before the congress of German naturalists, presumably hoping to obtain an equally decisive result.

But the problem remained Grant and Blainville, and because Grant was a serial transformist it became imperative to dispose of his views as publicly as possible. Buckland determined to get Grant's heretical view 'officially' discredited. First, he asked Owen to counter Blainville's criticisms,* lending him the two Ashmolean jaws for the purpose (104).

(though recognizing it as an unfortunate choice (96)), then switching back to Blainville's *Amphitherium* in his 1842 BAAS Report on British fossil mammals (97). This confusion did have its lighter side. The *Athenaeum* had followed the debate with great interest and reported on the see-sawing fate of the tiny jaws in its "weekly Gossip" column (98). To "avoid making an invidious selection of the different claimants to the right of christening", it renamed the beast "*Botherationtherium*", later *Botheratiotherium* (99). Blainville, whose English was presumably none too good, evidently missed the joke, and in the prestigious *Comptes Rendus* protested at this infraction of zoological rules. Needless to say, the *Athenaeum* saw the funny side of Blainville's reply (100).

*Owen's personal relations with Blainville are not clear. Appel only comments that they carried on a "friendly" correspondence (101); however, Owen's correspondents in Paris held distinctly disparaging views of Blainville's abilities, and freely communicated them to Owen and Clift. Pentland told Clift in 1832 that Blainville, despite succeeding to Cuvier's Chair, "had abandoned in a great measure Comp. Anat. for the last 16 years, & that he is now too old, too idle, and too stubborn to recommence the [illegible] of the Science" (102). Cuvier's sycophant C. L. Laurillard (1783-1853) also complained to Owen of Blainville's unreasoning criticism of Cuvier and appalling lack of logic (103).

Owen was the perfect choice: he was the foremost expert on marsupials in London, was moving into palaeontological studies from a ‘Bucklandian’ perspective, and he was keenly aware of the theoretical implications of the “saurian hypothesis” of the Stonesfield jaws and its support for the progressive theory. He first answered Blainville at the ZS in October, when discussing the osteology of marsupials. Here he probably encountered little opposition (105), Grant having withdrawn, even though the Society’s Secretary William Ogilby was sceptical. A few weeks later he took the issue before the GS where Grant *was* present. Since Owen also had access to Broderip’s fossil, by now deposited in the British Museum, and Colonel Sykes’ specimen in York Museum, his study was based on all four originals. In the first part of his paper to the GS in November 1838 he concluded from the convex condyle that these definitely were fossil mammals. Then he pointed to George Waterhouse’s newly-discovered numbat *Myrmecobius* as the most closely comparable living marsupial. Waterhouse had described two specimens of long-jawed numbat recently received from the colony* in 1836 as “analogically” comparable “to the genus *Tupaia* among the true *Insectivora*” (106): in other words, it was an insect-eating shrew-like marsupial, with *nine* molars in each half jaw (cf. *Thylaco-*

* The advantages of colonial imports in pushing British comparative anatomy up to and beyond French standards cannot be overemphasized. Blainville, Valenciennes, etc., were still not using the shrew-like numbat from the Swan Settlement in their 1838-9 studies, relying, like Cuvier before them, on familiar museum specimens of the opossum *Didelphis*.

therium Prevostii with ten). Owen urged that this living marsupial “decisively proves ... that [the Stonesfield animals] belonged to a true warm-blooded, mammiferous species” (107), and he retained Valenciennes’ name because he too saw no ambiguity.

This is what Buckland wanted to hear. But Owen encountered heavy resistance at Somerset House according to *The Athenaeum’s* gossip column. His paper

was followed up by a protracted and brilliant discussion. The laws of etiquette in force at the Geological Society, forbid us to notice either persons or opinions, in reference to the debates which arise at the assemblies of that body, and we can therefore only intimate, in general terms, that the result was more favourable to the views of M. de Blainville than we were prepared to expect; perhaps, indeed, sufficiently so to prove the impossibility of solving the problem, without having recourse to the chisel and hammer in the quarries of Stonesfield (108).

Grant’s was undoubtedly the main voice raised in opposition (1091). The only other known opponent was the Secretary of the ZS, William Ogilby, but in a communication on 19 December he simply listed the features for and against a marsupial relationship and concluded almost non-committally that so many of each existed that it was impossible to say definitively to which class the fossils belong (110). Even then, he told Owen that his anti-mammalian objections had been expressed in the published “abstract more strongly than he intended” (111). So the main opposition rested with Grant, and its force stemmed from his commitment to the “‘progressive’ theory”, as *The*

Athenaeum dubbed it (112). Buckland was possibly as surprised as the *Athenaeum* reporter by the heated response, hence his attempt to persuade influential parties to attend the reading of the second part. He wrote to Brougham* on 26 November, sending news of his own visits to footprint sites, adding:

I have a communication [on fossil tracks] to be read at the next meeting of the Geological Society on Wednesday the 5th of Dec at 9 p.m. when we shall also have a paper by Mr Owen on Blainvilles Botheratiotherium. Should Your Lordship have time and inclination to attend I shall be most happy to introduce you & shd. you be disengaged & disposed to meet Sedgwick Darwin Greenough Murchison Lyell & some more of the élite of the Society at the Dinner of the Geological Club on the day of the Meeting, at the Crown & Anchor at 6 precisely ... I shall be highly gratified by the honour of Your Lordships Company on that Occasion (119).

The second part of Owen's paper, on his newly-named *Phascolotherium (D. Bucklandii)*, was not in fact ready by the 5th, and he did not attend that week. After the meeting Buckland dropped him a revealing letter:

I will thank you to inform me as soon as possible whether you will have ready your paper on Broderip's

* Buckland had corresponded somewhat deferentially with Brougham for many years. While Canon of Christ Church he had sought His Lordship's patronage in aid of a second living (113) and the Radcliffe Librarianship (114). Buckland also acted as intelligence gatherer, sending papers on palaeontological matters (115) and passing on news of discoveries (116); and he corrected the proofs of Brougham's geological works (117), and persuaded him to add his name to subscription lists of seminal works, like Agassiz's *Fossil Fishes* (118). Thus there was a complex two-way interaction. The rewards were evident: Buckland could politely suggest to Brougham that he might like to attend particular GS meetings. Actually Brougham was *au fait* with the Stonesfield debate and keen to come; but one can understand Buckland's eagerness if a political *coup* at the GS was in the offing.

Stonesfield jaw by the next meeting of Geol Soc on the 19 as I know Lord Brougham is interested about the Botheratiotherian & if your paper will be read I will invite him to the meeting. Your absence was felt at the meeting of the 5th when we had the Chirotherium footsteps which I believe after all to be Reptile. Sir P Egerton will have told you why. Dr Grant seemed disappointed that he could not differ from me. I think it desirable for the sake of everybody both in London & Paris to put the Marsupial Character of the Stonesfield beasts beyond all doubt as speedily as possible especially after what Grant has published in the Annuaire of Bailliere [i.e., Grant's *General View*] (120).

Owen's paper was ready to be read on the 19th, and Buckland again wrote to Brougham, inviting him to witness the triumph.

I learn from Professor Owen ... that his paper on Botheratiotherium Bucklandi will be ready for the Geological Society next Wednesday at 9. p.m. at Somerset House on which occasion we hope to give the coup de grace to those who would make a Reptile of this highly organized, tho early Representative of the mammals. We shall also have a Notice on more footsteps of Chirotherium at Shrewsbury and Warrington and Tarporley. I believe him to have been a Reptile. Should your Lordship like to witness our Skirmish between 9 & 11 or to dine at the Geological Club at 6 Piccadilly at the Crown & Anchor I shall feel highly honoured by your company. I believe Whewell will be at the Dinner[,] certainly not Sedgwick. shd Your Lordship dine I will invite Owen (121).

So there is no doubt that Grant was seen as the main protagonist and that "skirmishes" were anticipated and materialized, with no doubt the "élite" diners of the Geological Club massed against him – and that presumably included Darwin (122).

All that remained was to invalidate the last major piece of evidence appealed to in support of the "saurian hypo-

thesis” – the reptilian diagnosis of the American *Basilosaurus*, with its complex roots like those in the Stonesfield jaws. At the meeting on the 19th Buckland and Owen evidently conspired on this point, because Buckland wrote on 4 January:

Our last talk about old Nick has, as usual produced his Horns. The first thing I did on my return from the last meeting of the G.S. was to begin a paper to show that *Basilosaurus* was a true aquatic mammal, commencing with a statement of my reasons for entering on this, questioning the use lately made by Blainville & Grant of Dr Harlan's paper to support their notion of the Stonesfield Mammals being Reptiles (123).

The Quaker Richard Harlan (1796-1843) was a seventh generation American from a distinguished family (124). He had gained an MD from the University of Pennsylvania and at an early age was elected Professor of Comparative Anatomy in the Philadelphia Museum (1821). Patsy Gerstner rates him one of the most skilful American palaeontologists of the 1830s, and her study has increased our understanding of Harlan's role in the two-way transmission of palaeontological ideas across the Atlantic at this time. Harlan distributed his *Medical and Physical Researches* (1835) and actively publicized his discoveries among the savants of Europe. He ingratiated himself with the English elite, sending boxes of fossils to Murchison, for example, and having the imperial geologist elected to his own Geological Society of Pennsylvania (125). Harlan's book contained a lengthy description of the *Basilosaurus*, an anomalous saurian with complex-fanged teeth, which Grant and Blainville pointed to in support of a reptilian *Amphitherium*. Early in January 1839, in the midst

of the Stonesfield controversy, Harlan arrived in London packing basilosaur bones from the Alabama plantations. This unexpectedly gave English geologists an opportunity of examining the fossil and controverting his diagnosis. (Buckland had already made up his mind that it was a whale – probably an idea originating with Owen.) In a letter to Owen Buckland made clear the tactical advantage in persuading *Harlan* himself to recant publicly and thus swing the vote. Because of Harlan's arrival Buckland deferred his own paper, and he suggested that Owen work on Harlan to extract a recantation. Failing that Buckland offered a number of alternative strategies to defeat the opposition:

The arrival of the originals makes me doubt the propriety of saying anything about them, at least at the next meeting when the Dr will be present, but on this point I shd be glad to have your judgment & if you see Dr H. I will thank you to sound his feelings on the matter as if he is to be overhauled, or rather his paper to be criticised it will be more fair to do it in his presence & with the bones on the table, than in his absence. If he does not fight shy I will promise to be more courteous to him than I was at Lanobridge when I attacked him not knowing who he was. As I shall not have seen the bones I can only offer my observations on his presented paper & state my reasons for believing the animal to be no reptile but a mammal from his shewing in that paper. If the bones are on the table & I presume they will be some sort of Notice shd be read in order to draw attention to them, & get the fact recorded in the Report. The best thing wd be 2 or 3 pages of recantation by himself, if you have convinced him of his error. The next best thing will be a short paper by you founded on the specimens you have examined, the 3d alternative will be a statement of my reasons for dissenting from his published paper. Till I hear from you I will make no further progress with what I have begun. I shd do little more than state in writing what I uttered in words at the meeting, when Stonesfield beasts were on the *[illegible]*. We had better be guided in all this by Dr. H.'s own feelings.

Do persuade him if you can to sign his own recantation it will be the most agreeable to his feelings, most honourable & most influential way of setting the matter right.
...nothing but Dr Harlan's arrival with his bones wd have induced me to come up on the 9th (126).

Buckland's intention was transparent. It was to turn the basilosaur bones against those who had appealed to them in support of the "saurian hypothesis" and ensure maximum publicity for the rout. He added a PS:

Some how or other we must contrive to anticipate on the 9th what will otherwise be done with the Basilosaurus at Paris & promote him to the Rank of a mammal & get the promotion gazetted in the Report of the Geol. Society.

It is obvious that there was considerably more at stake than simply correct interpretation. Buckland's machinations stemmed from his sensitivity to the ideological threat and realisation of the consequences should the outcome go against himself and Owen. The ruse evidently worked. Although Grant introduced Harlan as *his* guest at the Society on 9 January (127), the letter Harlan then read must have come as a grave disappointment (particularly if Grant had counted him an ally). Harlan told the President (Whewell) that he had first thought the bones to be those of a marine carnivore, although closer study of the lower jaw convinced him of its saurian nature. The immense size of the vertebrae, the peculiar ribs, humerus, etc., "all differ widely from those parts in any of the known species of the Cetacea." But he continued: "I nevertheless take pleasure in acknowledging that the accurate and labourious investigations, made by my friend Mr. Owen on

these fossils, during the several days they remained with him at the Royal College of Surgeons, have thrown new light on their structure and analogy" (128). Owen had evidently persuaded Harlan to allow him to section and microscopically examine the teeth some days previously; and at the Society he followed Harlan with a paper on the *cetacean* nature of the bones and teeth, dismissing this last vestige of support for the "Saurian hypothesis" of the Stonesfield jaws (129).

We can now go beyond Gerstner's analysis of the incident. She portrayed Owen acting in "consultation with Harlan" in the interests of Transatlantic co-operation. As she says:

Owen made it clear to members of the Geological Society of London, and through their publication to the learned community in general, that he and Harlan had arrived at the conclusion together after a joint examination of the specimen. In this cooperative effort palaeontology as a transatlantic pursuit emerged full-grown (130).

If Owen indeed managed to create this impression then it was a job well down. But the letters show that it was not an attempt to further diplomatic relations. Owen and Buckland had aimed to create just such an impression; they conspired to present the Society dissidents with a *fait accompli* – Harlan, the guest, fully converted at his first appearance. Thus Owen's *mode of presentation* was strategically important. It was less an exercise in international good-will than an earnest attempt to regain the co-opted fossils and turn them to anti-serialist ends.

Financing and Patronage: The Social Rewards of Acceptable Science

How Grant reacted to the *coup* is difficult to say. Caroline Owen recorded in her diary: “R. to the Geological Society, where he read the paper on Dr. Harlan’s fossil and the Stonesfield jaw. Dr. Grant was obliged to admit, in spite of his teeth, that they were mammalia and not saurians” (131). One has to be inordinately careful of statements in Owen’s *Life*; Caroline’s notoriously uncritical reporting (she was not there, but was repeating what Richard told her) makes it prone to constant exaggeration. That Grant *did* have difficulty accommodating a Stonesfield *marsupial* is suggested by his tortured comments in the subsequent “Palaeozoology” lectures (Ch. 8).

Most likely it hardened him against Owen; certainly from this time we begin to hear of their angry public exchanges. Grant did not withdraw from Somerset House as he had done from Bruton Street. The Ordinary Minutes testify to his constant appearance with guests throughout the 1840s (132). But we know little more of the machinations of the Society elite, insofar as they affected him. The only subsequent event of importance was in 1842 with the altercation following the reading of his mastodon paper. Again, because of the Transatlantic connection, this subject has been treated by Gerstner, who concludes that the exchanges at the

GS over the correct interpretation of Koch's 'Missourium' set up in the Egyptian Hall in Piccadilly reflected the disagreements of palaeontologists in America (133). So far as I can tell, the episode this time *was* one solely of correct interpretation (although with Grant's "Mastodon" MS lost this can never be entirely certain). I will concentrate on aspects of the dispute not covered by Gerstner to point up the problems Grant was now facing. The cause of the *fracas* was a giant proboscidian, interpreted in America variously as *Tetracaulodon* or *Mastodon giganteum*. In America in 1841 Lyell went with Harlan to see Koch's monster, which was restored higgledy-piggledy, with spurious and misplaced bones. He considered Koch "amusingly ignorant" and wrote home to Mantell that he would have a great treat as Koch was bringing the show to London (134). Mantell went to see the huge elephant when it arrived in Piccadilly and confirmed that it was "miserably put together" (135). Owen diagnosed it as *Mastodon giganteum* while Grant, Koch, and the amateur geologist Alexander Nasmyth considered it *Tetracaulodon*. All four men delivered papers at the Society between February and June 1842 (Koch being introduced as Grant's guest (136)), and Mantell recorded that an "angry discussion between Owen and Grant" followed the latter's presentation on 15 June (137). In the *Proceedings* Owen's paper ran to over six pages; Grant's was merely printed as a one-side abstract. Yet he had in fact taken several months to complete a definitive monograph on the osteology of the mastodontoid genera. The abstract did not mention the size of the original memoir,

only that it was divided into eight sections, and dealt with the development, form, structure, and dental changes of the principal genera relative to age, sex, and species. From the abstract, it appears that Grant had gone into the question of the relationship of the tusked *Tetracaulodon* to the immature *Mastodon* more searchingly than Owen, and from Mantell's report he was not afraid to stand up to Owen at meetings. The whole issue rankled enough for Wakley to record in the "Biographical Sketch" that Grant had

identified and catalogued every fragment, executed full-size drawings of nearly thirty of the most interesting specimens; and, at the request of the President of the Geological Society, drew up a Memoir of more than 200 quarto manuscript pages, of the results of some months' examination of these gigantic fossils. Matters of science, like those of politics, have their moment of novelty ... The Memoir proved too long to be read at the Society; the subject was of too little popular interest to allow his publisher to risk its publication, and the author had *too much neglected Mammon and the capricious Dame to be able to undertake it himself*

While the memoir was probably not "suppressed", publishers would surely have been less inclined to take the risk after the GS had turned it down, and Grant was in no position to print it himself. (The costs, e.g., for printing Owen's long memoir on the Megatherium a decade later were estimated at £700 (139).) So at the very least the moral might be that Grant's increasing pecuniary embarrassment was affecting his ability to *publicize* his science to the extent that Owen now was.

Owen gained immeasurably from his performance at the GS. Financed by the managers of the BAAS in 1841, he could draw up a Report on British fossil mammals which rehashed the whole Stonesfield affair. (The only change was the inclusion of a new Jurassic jaw with *twelve* molars, suggesting that *T. Prevostii* was possibly a placental insectivore (140).) He then extended this Report into his influential *History of British Fossil Mammals*, which was tactfully dedicated to Murchison (President of the BAAS), and proof-read by Broderip. He reslayed the slain to great effect, giving a blow-by-blow account of the Stonesfield proceedings of 1838-9, quoting in its entirety Grant's published account in *General View* before systematically demolishing it (141). By now sure of himself, Owen mooted the former presence in Britain of an entire Australian-like fauna, including thylacines and dasyures to prey on the shrew-sized amphitheres and phascolotheres – a fauna “superseded in our hemisphere” by higher mammals but persisting little changed in the colonial outpost in the antipodes (thus posing problems of “marsupial” distribution which were to puzzle palaeontologists for four decades (142)).

But the real pay-off from Owen's co-operation with Peelites like Buckland and Whewell was in terms of career enhancement and pecuniary aid. Today – a decade after Roy MacLeod was forced to apologise that it was “not quite ‘proper’” to inquire into a scientist's financial circumstances (143) – it

is less unfashionable to interpret patronage as a means of encouragement and as a reward for doing acceptable science, and as an incentive to keep scientific doctrines within the strict limits set to comply with contemporary political or cultural standards. MacLeod is one of the most sensitive historians of scientific endowment and has been instrumental in pointing out the larger territory waiting to be conquered by social historians. It might sound a truism that “the rate and direction of research in natural science is partly dependent on the influence of external social, political and economic considerations” and that “economic factors may impel particular fields of research or innovation” (144); but it has proved difficult to detail individual cases where patronage is employed for overtly political or social ends. Morrell and Thackray in *Gentlemen of Science* have demonstrated how successful social control of an association through the allocation of funds could be. We have already noted the BAAS managers’ diversion of funds to Owen in this period (1838-41). Owen was without question sensitive to the spirit in which the awards were given, and aware of what was expected of him. Hence his BAAS papers were tactical masterpieces: long, scrupulously documented attacks on Lamarckism and the “progressive” theory, and vindications of the socially-stratified moral cosmos on which Oxbridge divines rested their temporal control of science. The patronage Owen received testified magnificently to his having amply met establishment requirements. It was no coincidence that his rapid social elevation followed so closely on the success of

his anti-transformist campaign at the BAAS and GS. He was also now able to exploit his *entrée* into privileged Oxbridge society provided by admirers like Buckland and Whewell. They had the ear of Sir Robert Peel, the new Prime Minister (1841-7). Indeed, with the collapse of Lord Melbourne's Liberal ministry in 1841 and Peel's return to Number 10, Whewell was offered and accepted the Mastership of Trinity as a political gift (145). Peel was more favourably disposed towards men of science in terms of the Civil List pension (146), and Owen used his new political connections to canvass on his own behalf. Yearly pensions were limited to £1200 in total, and Peel fought for their distribution to persons "who have either rendered a service to the public" or who "have rank and title and no means of maintaining them to live at least in dignity" (147). While he obviously had in mind destitute gentlemen needing to keep up a respectable appearance, there was no reason why this criterion should not apply, *mutatis mutandis*, to titular heads of science wanting an income commensurate with their station. This is how Owen chose to present his case. He reported back to Caroline from St. John's College, Cambridge, two days after Christmas 1841: "I have met, at Dr. Whewell's, the present Chancellor of the Exchequer [Henry Goulburn (1784-1856)] & Lord Brougham and have represented to them my present anomalous position, holding a Cuvierian rank without the means of doing it justice" (148). He acquainted Goulburn and Brougham with the meagre remuneration allotted to "the investigators of natural

history” and made his case for a pension of £200 or £300 p.a.

While pensions after the Civil List Act of 1837 could no longer be looked upon as party gifts (although Peel and the Radicals had voted against the Act: Peel to keep the award a ministerial rather than Parliamentary prerogative, the Radicals objecting on anti-elitist ground), Peel on coming to office was nonetheless advised by aides like Buckland on the scientific merits of claimants. Hence within a fortnight of meeting the Chancellor at Whewell’s, Owen had written a long letter to his friend Buckland pressing his claim. To understand his presentation we must realise that, for scientists at least, the pension now served – as Herschel put it to Peel at this time – to relieve “men of a very high order of attainment, and who have distinguished themselves for original research, during those years *while their powers are still unimpaired and available for discovery*, from the necessity of looking either to public or private instruction as their chief means of support” (149). It was to be used as an incentive to men of the highest calibre to continue working free of worry. Hence Owen’s careful phrasing as he laid his grandiose plans before Buckland early in January 1842. He explained that he expected to fail in his future tasks for financial reasons and portrayed this as the country’s loss:

I have long cherished the design to embody all the information which I have acquired by observation and reading in animal organization into the form of a general Treatise in Comparative Anatomy, with the

rquisite illustrations. And since the fate of Cuvier who died ere he could commence a similar work promised from the beginning, and of Meckel who left the same design two thirds unfinished warn me that it demands more than is Man's to [?give, I propose] to complete such a work in a series of separate Treatises each having one system of organs for its subject, & complete in itself; of which the Odontography will be a sample: this to be followed by an Osteography, Neurography, Genetography, &c: so that if I leave the whole plan unfinished the purchasers of the completed part will have one or more entire books instead of an unfinished work. More obstacles have, however, offered themselves to the entertainment of such a design that at first appeared. Although I have found a bookseller willing to take the expense of publishing the first Treatise, yet the preparation & acquisition of the materials – sections of teeth – has compelled me to practise a rigid economy. Increasing expenditure has been the unavoidable result of the position in society to which my scientific labours have raised me; and my dear lad has grown tall enough to remind me that I must lay by for the expenses of his education. Longman, on the other hand, offers a tempting bait if I will consent to the compilation of a Dictionary of Anatomy & Physiology; but I fear that the energies which my favourite work demands, would be dulled if not exhausted after the drudgery, of a four or five year's task-work. I am unwilling that England should lose the credit of producing that Work on Comparative Anatomy, which France & Germany have, as yet, failed in achieving; and, believe me, I indulge in no less hopes than the completion of such a survey of the highest class of created things on this planet as will be recognized to be the parallel of that which Cuvier & Meckel have attempted to give (150).

Owen unashamedly traded on his friendship with Buckland and particularly on his Oxbridge reputation. He referred Goulburn (the Conservative MP for Cambridge) to "Whewell, to the Professor of Anatomy & Medicine, and of Geology in the Universities of Oxford & Cambridge, for testimony of the nature, extent and disinterested appliance of nearly twenty years researches in Physiology and Comparative Anatomy". Cambridge divines had convinced Owen that "science would be benefited

by the favourable consideration” of his claims, but then Anglican science stood to gain most from his continued anti-transformist exertions. His Oxbridge testimonials had an obvious use beyond coming from some of the better known cultivators of science. Owen had wedded his claim to national prestige; Buckland now amplified this in a long accompanying letter to Sir Robert. He used the Premier’s desire “to promote the scientific reputation of this Country” to extol the “transcendent merits” of Professor Owen, the “chief Comparative Anatomist of England” (151). He emphasised Owen’s age (“about 40”) and “high qualifications” for carrying out his ambitious plans. As testimony of Owen’s competence, he reminded Peel that the BAAS had awarded him grants “from year to year”. But it was primarily Owen’s position, potential, and respectability that Buckland played up, exaggerating his financial straits to an appalling degree:

At a meeting of the naturalists of Germany, at which I also attended, at Freyberg in 1839 Mr Owen was considered the first among many assembled representatives of European science; and in our own Country his position is quite as high as that of Airey [sic], Faraday or Dalton. He has abandoned his prospects in the medical profession to devote his life to the advancement of science, and since the death of Cuvier even France herself has looked up to Owen as the only worthy successor of that great man. It would be indeed a national reproach & an irreparable loss to the World of Science if the possessor of such unrivalled talents & acquirements were obliged to descend to the Condition of a Bookseller’s hack, when an addition of £300 a year would secure the devotion of his whole life, now in its prime, to the probable completion of a career of more original research, & more comprehensive views of the Totality of organized life than any human being has yet had talents combined with opportunities sufficient to accomplish, and I have peculiar satisfaction to add that his opinions on religious matters are sound & temperate;

& that every new discovery he makes excites in him such feelings as a mind constructed like that of Paley is alone competent to enjoy.

When Peel came into office he found that the amount set aside for the Civil List that year had been exhausted by the outgoing government (152). But he did ensure that when he received the new grant in 1842 Owen was awarded the lion's share of the amount set aside for scientists, taking £200 of the available £300 (153). Whewell, on being told the news, disclaimed any credit (only admitting that he had mentioned Owen's name in quarters which might have had "conceivable influence"), and told Owen that the "well deserved honour" would protect him from the "molestation" of less appreciative scientists (154). He admitted that the amount would do much "added to your other resources to place you in a condition to pursue your researches at your ease". Although naturally enough Owen promised Peel that "the handsome provision" would "enable me to pursue my studies with renewed ardour and to show by increased exertion my gratitude for the royal favour" (155).

Of course Owen never was in any danger of becoming a "Bookseller's Hack", but the additional income enabled him to pay off the draughtsmen and bring out his *History of British Fossil Mammals* (issued in twelve parts between 1844-6). This was a speculative publishing venture. His expenses were £1000, of which the BAAS contributed £250; he was printing 350 copies and was "sanguine enough to expect no loss" (156).

In an age when serious publishing cost an author money (157), the combined grants, awards, and pension allowed him to publicize his science effectively and in a more lavish way than an out-of-favour radical like Grant found possible. At the same time Owen's own political influence began increasing. He entertained Peel and Prince Albert (both in the company of Buckland) at the Hunterian Museum in 1843 (158). And within the year Owen was himself a guest at Drayton Manor, arranging for his portrait by Pickersgill to be hung alongside Cuvier's in Sir Robert's gallery (159).

Discussion of Owen's social rise beyond this point is not germane, even if the subject has suffered historiographical distortion, partly from Reverend Owen's attempt to inveigle grandfather into the highest echelons in the official *Life*. But mostly the caricature stems from the perceptions of T. H. Huxley, Herbert Spencer, and others among the young guard of the 1850s, whose middle-class 'mercantile' interests left them profoundly unsympathetic to Owen's Oxbridge connections and associated idealism (a historiographical problem I have taken up more fully in *Archetypes and Ancestors*). I would simply finish by suggesting that for a more balanced appraisal of Owen's social position and scientific responsibilities, the Owen letters at South Kensington and Lincoln's Inn, and Peel-Owen correspondence in the British Library, might be consulted. These leave a more rounded impression by highlighting Owen's work on Chadwick's Sanitary Commission and his efforts to improve the scientific

collections, especially his agitation for the establishment of a “National Collection of Comparative Anatomy” at the College of Surgeons (160). Finally, while I have demonstrated how Owen’s reaction in the troubled thirties might be interpreted partly as an institutional response to radical transformist attacks, I believe that it is unprofitable to defend the prevailing Huxleyan caricature of Owen as an obstructionist villain, or worse, a “social experimenter with a penchant for sadism and mystification” (161). This misapprehends the social conditions for the patronage of science in the 1830s, and it ignores the subsequent productivity of Owen’s archetypalism (for example, in palaeontology). It is fairer and more historically relevant to view Owen’s comparative anatomy in contemporary political terms. Like a Peelite Conservative at the time of the *Tamworth Manifesto*, he sought to safeguard establishment interests by maintaining gentlemanly Anglican standards. Yet he too promoted a cautious and conserving type of reform. His anti-Lamarckian ideology was crucial to the generation of an Anglicized archetypal morphology and von Baerian palaeontology in the 1840s and these, historians are agreed, were among the most significant developments in pre-Darwinian comparative anatomy.

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36. Owen, op. cit. (33), f. 67.
37. Owen, op. cit. (14), f. 95; Owen, “Report on British Fossil Reptiles”, *Report of the British Association for the Advancement of Science* (Birmingham, 1839), 43-126 (46).
38. R. E. Grant, “Development of the Vertebral Column”, *Medical Gazette*, 14 (1833-4), 425-6; idem., “Lectures”, *The Lancet*, I (1833-4), 539-41, 572-4, passim; idem., *Outlines of Comparative Anatomy* (London, Bailliere, 1835), 57. Cf. Owen’s commentary on Grant’s defence of Geoffroy’s vertebral osteology: Owen, “Report on the Archetype and Homologies of the Vertebrate Skeleton”, *Report of the British Association for the Advancement of Science* (Southampton, 1846), 169-340 (232, 241 note, 253).
39. Owen, op. cit. (14), f. 95.
40. Ibid. ff. 96-7.

41. Ibid. f. 97.
42. Ospovat, op. cit. (12), 130-2.
43. R. Owen, *Lectures on the Comparative Anatomy and Physiology of the Invertebrate Animals* (London, Longman, 1843), 367.
44. Ibid. 370.
45. Ibid.
46. In op. cit. (36), 232, Owen mentions Grant as Geoffroy's disciple on the question of opercular analogies.
47. Owen, op. cit. (14), ff. 97-8.
48. Ibid. f. 98.
49. Oppenheimer, op. cit. (17), 230.
50. C. Lyell, *Principles of Geology* (London, Murray, 1830-3), ii, 62.
51. Ibid. 63-4.
52. D. Ospovat, "Darwin and Huxley on Divergence" MS, f. 10. P. J. Bowler, *Fossils and Progress: Paleontology and the Idea of Progressive Evolution in the Nineteenth Century* (New York, Science History Publications, 1976), 110.
53. R. Owen, *A History of British Fossil Mammals and Birds* (London, van Voorst, 1846), 35.
54. K. Lyell (ed.), *Life, Letters and Journals of Sir Charles Lyell* (London, Murray, 1881), ii, 37, 39.
55. Rev. R. Owen, *The Life of Richard Owen* (London, Murray, 1894), i, 122.
56. Ibid. i, 90. Mrs Clift, ambitious for her daughter and new son-in-law, wrote to Caroline on honeymoon that her "meeting of the great Lord *Chief Justice* and his civility" and "your very kind reception by Dr and Mrs Buckland is extremely pleasing to me": C. H. Clift to C. Owen, 22 July 1835, BL Add. MS, 39,955, f. 225. Well before that time Owen was making experiments for Buckland (Rev Owen, ibid. i, 66), and Buckland had also tapped Owen's brains on Didelphis and marsupial generation: W. Buckland to R. Owen, 25 January 1835, BM(NH) OC 6.116.
57. W. Buckland to R. Owen, 9 March 1838, RCS MS (1)a/li.
58. Ibid.

59. W. Buckland to R. Owen, 24 February [1839], RCS MS (1) a/19.

60. Geoffroy St.-Hilaire, “De Quelques contemporains des Crocodiliens fossiles des ages antédiluviens”, *Comptes Rendus de l'Académie des Sciences*, 7 (1838), 629-33.

61. For overviews of the subject see R. Owen, *Monograph of the Fossil Mammalia of the Mesozoic Formation* (London, Palaeontographical Society, 1871); H. de Blainville, “Doutes sur le prétendu Didelphus fossile de Stonefield”, *Comptes Rendus de l'Académie des Sciences*, 7 (1838), 402-18.

62. W. J. Broderip, “Observations on the Jaw of a Fossil Mammiferous animal, found in the Stonesfield slate”, *Zoological Journal*, 3 (1827), 408-18.

63. W. Buckland, “Notice on the Megalosaurus or great Fossil Lizard of Stonesfield”, *Trans. Geol. Soc.*, 1 (1824), 390-6 (391).

64. C. Prévost, “Observations sur les schistes oolithiques de Stonesfield en Angleterre dans lesquelles ont trouvés plusieurs ossemens fossiles de mammifères”, *Ann. des Sci. Nat.*, 5 (1825), 389-417. Appel (op. cit. 20), 317.

65. G. Cuvier, *Recherches sur les Ossemens Fossiles* (Paris, 1825), v, 349.

66. Broderip, op. cit. (62), 411.

67. Fitton contributed to Broderip, ibid. Lyell and Phillips are also mentioned as working on Sykes’ specimen in A. Valenciennes, “Observations sur les mâchoires fossiles des couches oolithiques de Stonesfield”, *Comptes Rendus de la Académie des Sciences*, 7(1838), 572-80 (575).

68. W. Buckland, *Geology and Mineralogy* (London, Pickering, 1837), i, 72.

69. Ibid.

70. Owen, op. cit. (30), 203-4.

71. Geoffroy St.-Hilaire, “Le degré d’influence du monde ambiant pour modifier les formes animales”, *Mémoires de l'Académie Royale des Sciences*, 12 (1833) 63-92.

72. Valenciennes made this point, op. cit. (67), 573.

73. R. E. Grant, “Lectures”, *The Lancet*, 2 (1833-4), 72.

74. Bowler (op. cit. 52), 20-1.

75. See especially Owen, op. cit. (53), 3742, and Owen, op. cit. (61), 13, where Grant figures strongly in Owen's reconstruction.

76. Cf. Blainville, op. cit. (61), 405; Blainville, "Nouveaux doutes sur le prétendu didelph de Stonesfield", *Comptes Rendus de l'Académie des Sciences*, 7 (1838), 727-36 (729); and Valenciennes (op. cit. 67), 573.

77. Blainville, op. cit. (61), 405; and idem., "Nouveaux doutes", ibid, 729.

78. Blainville, op. cit. (61), 405, 418; "Nouveaux doutes", ibid. 729, 730.

79. Blainville, "Nouveaux doutes", op. cit. (76), 730.

80. Blainville, op. cit. (61), 416.

81. Ibid. 417.

82. Blainville, "Nouveaux doutes", op. cit. (76), 736.

83. R. E. Grant, *General View of the Characters and the Distribution of Extinct Animals* (London, Bailliere, 1839), 7, also 54.

84. Ibid. 42-3.

85. Ibid. 44-5.

86. Buckland, op. cit. (68), i, 265 note.

87. On their discovery see "An account of Footsteps of the Cheirotherium, and other unknown animals lately discovered in the quarries of Storeton Hill", *Proc. Geol. Soc.*, 3 (1838-42), 12-14 (12).

88. "Scientific Intelligence", *Mag. Nat. Hist.*, 3 (1839), 43-8 (43).

89. R. E. Grant, "On a Fossil Tooth found in a Red Sandstone above the Coal Formation in Berwickshire", *ENPJ*, 16 (1834), 38-43.

90. R. E. Grant, "Antediluvian Remains at Stourton Quarry," *Liverpool Mercury*, 24 April 1838; *Mag. Nat. Hist.*, 3 (1839), 43-8 (44).

91. Ibid. 46.

92. Op. cit. (87), 14.

93. Grant, op. cit. (90), 48.

94. Valenciennes, op. cit. (67), 572.

95. Blainville, “Nouveaux doutes”, op. cit. (76), 729.

96. R. Owen, “Observations on the Fossils representing the *Thylacotherium Prevostii*, Valenciennes, with reference to the Doubts of its Mammalian and Marsupial Nature recently promulgated; and on the *Phascolotherium Bucklandii*”, *Trans. Geol. Soc.*, 6 (1842), 47-57 (57) [read 21 November 1838].

97. R. Owen, “Report on the British Fossil Mammalia. Pt I”, *Report of the British Association for the Advancement of Science* (Manchester 1842), 54-74 (62).

98. *The Athenaeum*, Nos. 570, 572, 578 (1838), 731, 747, 841.

99. Ibid. 731, 841.

100. Ibid. 841. Blainville, “Nouveaux doutes”, op. cit. (76), 735.

101. Appel, op. cit. (20), 316.

102. J. Pentland to W. Clift, 5 November 1832, copied by Owen in his Notebook 9 (1832-3), BM(NH), f. 90.

103. C. L. Laurillard to R. Owen, 12 October 1843, BM(NH) OC 17.205.

104. Owen, op. cit. (96), 47.

105. None is mentioned in *The Athenaeum*, No. 572 (1838), 747.

106. G. R. Waterhouse, “Description of a New Genus of Mammiferous Animal from Australia, belonging probably to the Order Marsupalia”, *TZS*, 2 (1841), 149-54 [read 13 December 1836]. Waterhouse was Curator to the ZS.

107. Owen, op. cit. (96), 57.

108. *The Athenaeum*, No. 578 (1838), 841.

109. I assume that as the leading London protagonist he was present at the meeting. The Ordinary Minutes of the GS only record those occasions when a guest was introduced; hence he is recorded as signing in guests on 5 and 19 December, and 9 January 1839: Ordinary Minute Book 9, Geological Society MS.

110. W. Ogilby, “Observations on the Structure and Relations of the Presumed Marsupial Remains from the Stonesfield Oolite”, *Proc. Geol. Soc.*, 3 (1839-42), 213 (23).

111. Owen, op. cit. (53), 37.
112. *The Athenaeum*, No. 570 (1838), 731.
113. W. Buckland to Lord Brougham, 14 June 1832, UCL Brougham Papers, 20,098.
114. Buckland to Brougham, 28 January 1834, *ibid.* 46,563; 2 February 1834, *ibid.* 46,809.
115. Buckland to Brougham, 26 November 1838, *ibid.* 20,100.
116. Buckland to Brougham, 23 December 1838, *ibid.* 20,104; 26 March 1839, *ibid.* 20,166 reporting on Sir Woodbine Parish's "Hog in Armour" (giant armadillo *Zyglodon*).
117. Buckland to Brougham, 26 November 1838, *ibid.* 20,101.
118. Buckland to Brougham, 29 February 1835, *ibid.* 20,099.
119. Buckland, op. cit. (115).
120. W. Buckland to R. Owen, 11 December 1838, RCS MS (1)a/6.
121. Buckland to Brougham, 14 December 1838, UCL Brougham Papers 1957.
122. It would be interesting to have Darwin's perceptions of events at the GS in December-January. He was in his most intense period of evolutionary theorizing, yet he was so clearly socially-aligned with Buckland et al (as is testified by Buckland's letter to Brougham). Although a staunch liberal from a free-thinking family, Darwin was first and foremost a wealthy gentleman amateur; Grant's radicalism, professionalism, and incomod existence were alien to him – nor would he have been sympathetic to Grant's Parisian serialism, being guided in such matters by the GS elite. One might imagine that, supported by this group, Darwin's fear of social betrayal stopped him from publishing his theory. Gruber's belief that Darwin refrained from fear of persecution for materialism would of course be a special case of this: H. E. Gruber, *Darwin on Man* (New York, Dutton, 1974).
123. W. Buckland to R. Owen, 4 January 1838 [1839], RCS MS (1)a/19.
124. A. H. Harlan, *History and Genealogy of the Harlan Family* (Baltimore, Lord Baltimore Press, 1914), 335; *Dictionary of American Biography* (New York, Scribner's, 1932), viii, 273.
125. R. Harlan to R. I. Murchison, 18 May 1832, 11 April 1834,

Geological Society, Murchison Correspondence.

126. Buckland, op. cit. (123).
127. Ordinary Minute Book 9, entry for 9 January 1839, Geological Society MS.
128. R. Harlan, "A Letter from Dr. Harlan, addressed to the President, on the Discovery of the Remains of the *Basilosaurus* or *Zeuglodon*", *Trans. Geo. Soc.*, 6 (1842), 67-8 [read 9 January 1839].
129. R. Owen, "Observations on the *Basilosaurus* of Dr. Harlan (*Zeuglodon cetoides*, Owen)", *ibid.*, 69-79 (69) [read 9 January 1839].
130. P. A. Gerstner, "Vertebrate Paleontology, an Early, Nineteenth-Century Transatlantic Science", *J. Hist. Biol.*, 3 (1970), 137-48 (147).
131. Rev. Owen, op. cit. (55), i, 152.
132. Ordinary Minute Book 10, Geological Society MS, *passim*.
133. Gerstner, op. cit. (130), 138-41. J. T. Gregory, "North American Vertebrate Paleontology", in C. Schneer (ed.), *Two Hundred Years of Geology in America* (Hanover, University Press of New England, 1979), 305-35 (307).
134. K. Lyell, op. cit. (54), ii, 59.
135. E. C. Curwen (ed.), *The Journal of Gideon Mantell* (London, Oxford University Press, 1940), 150.
136. Ordinary Minute Book 10, entry for 18 May 1842, Geological Society MS.
137. Curwen, op. cit. (135), 159.
138. BS 691; R. E. Grant, "On the Structure and History of the Mastodontoid Animals of North America", *Proc. Geol. Soc.*, 3 (1836-42), 770-1.
139. E. Sabine to T. H. Huxley, 30 October 1853, Imperial College, Huxley Papers, vol. 26, f. 6; quoted in Desmond, op. cit. (9), 28.
140. Owen, op. cit. (97), 57-62, 73-4. He now reverted to the name *Amphitherium*; Valenciennes' *Thylaco-* now being inappropriate, since it referred to the marsupial *Thylacinus*.
141. Owen, op. cit. (53), xiii-xv, 29-70 (esp. 37-42 for Grant).

142. Ibid. xiv; Owen, op. cit. (97), 74; Broderip, op. cit. (62).

143. R. MacLeod, “Science and the Civil List 1824-1914”, *Technology & Society*, 6 (1970), 47-55 (47).

144. Ibid.

145. Had the Whigs won the election it is possible that Sedgwick would have been offered the post. On the engineered aspect of this award see J. W. Clark & T. M. Hughes, *The Life and Letters of the Reverend Adam Sedgwick* (Cambridge University Press, 1890), ii, 35-6.

146. MacLeod, op. cit. (143), 48-50.

147. Quoted in MacLeod, ibid. 48.

148. R. Owen to C. Owen, 27 December [1841], BL Add. MS 45,927, f. 38.

149. Quoted in MacLeod, op. cit. (143), 50 (my emphasis).

150. R. Owen to W. Buckland, 11 January 1842, BL Add. MS 40,499, f. 252.

151. W. Buckland to R. Peel, 12 January 1842, BL Add. MS 40,499, f. 250.

152. R. Peel to W. Buckland, 19 January 1842, BL Add. MS 40,499, f. 254.

153. R. Peel to R. Owen, I November 1842, BL Add. MS 40,518, f. 24; Rev. Owen, op. cit. (55), i, 203-4.

154. W. Whewell to R. Owen, 9 November 1842, BM(NH) OC 26.283; Rev. Owen, op. cit. (55), i, 204-5.

155. R. Owen to R. Peel, 1 November 1842, BL Add. MS 40,518, f. 26.

156. Rev. Owen, op. cit. (55), i, 207.

157. H. Spencer, *An Autobiography* (London, Williams & Norgate, 1904), i, 393, on the cost of publishing and Owen’s primary consideration of whether he could afford the loss.

158. Rev. Owen, op. cit. (55), 211-2. In January 1843 Buckland warned Owen that it might be difficult for the Prince to slip quietly in Owen’s Museum, “but I shall try to get this done if it will not excite jealousy among your inmates, whose company would not be desirable”.

159. R. Owen to W. Buckland, 26 December [1844], BL Add. MS

40,556, f. 294; Rev. Owen, op. cit. (55), i, 244-7.

160. R. Owen to R. Peel, 20 April 1846, BL Add. MS 40,590, f. 113.

161. W. Irvine, *Apes, Angels, and Victorians: Darwin, Huxley, and Evolution* (Cleveland, Meridian, 1959), 38.

Chapter 8

Grant's Social and Scientific Decline: Some Conclusions

Grant's reformist opposition at the ZS, his resistance to the Peelite faction at the GS, and refusal to kow-tow to the managers of the RS and monopolists of the RCP – all these political moves were self-destructive in the sense of losing him scientific resources, institutional power and professional privilege. The failure of each radical demand resulted in greater personal hardship, compounded by the existing penalties of the *laissez faire* arrangements at the joint-stock university. Even his noted humour took a cynical turn. The *Medical Times*, itself dedicated to rooting out the “mammoth evils” of medical “misgovernment” (1), called Grant’s inflammatory address to the radical BMA in 1841 “a clever work by a disappointed man” (2) – a mixture of sublime expressions and appalling taste, of practical solutions perverted by “absurd and visionary” speculations. His “disappointment has soured him to the extent, we are sorry to say, of prejudicing his judgment”. It was a common enough criticism in the 1840s. John Beddoe encountered him in 1849, “a dry, melancholic, disappointed, humorous man, devoted to his subject with a burning zeal” (3). Grant’s financial plight grew desperate beginning in 1848 as attendance at his

university courses crashed. Whereas other professors were by now provided fixed annual incomes, for example Sharpey in 1846 had been guaranteed £600 by the college, “secured independently of all contingencies” (4), Grant was still reliant solely on fees. And during the early fifties his takings plummeted, often to around £30-£40 p.a. (5). They remained at roughly this low level throughout the 1850s and 1860s. Wakley was appalled at Grant’s “lapse into absolute penury” (6) and in the crunch year of 1850 devoted an editorial to his plight in *The Lancet*. He reported that, lacking any guarantee, Grant had not *the “slightest prospect of being able to make more than fifty pounds of income, DERIVED FROM EVERY SOURCE, during the year”*. Wakley harangued both the Exchequer for its lack of “state provision” for scientists and University College for its failure to avert such a financial “catastrophe”. As always Wakley saw the situation in cold political terms: Grant’s life

is not that of a physician accumulating his thousands per annum in successful practice; nor even that of one who, directing his energies to pursuits allied to medicine, has found means to acquire the patronage of dispensers of pecuniary emoluments, and of posts which he may richly deserve to fill. To the disgrace of Colleges, of corporate bodies, of the pretended patrons of learning, and of the Government under which we live, the case is quite otherwise.

Refusing to comply with the “illegal” demands of the RCP for re-examination, Grant was prevented by law from writing a prescription even “to save a brother’s life” (7). He was not rich like more moderate reformers, e.g. his friend Marshall

Hall, who had been making over £2000 p.a. from his practice since the 1830s (8).

Although by the late 1840s Grant was Dean of the Medical Faculty at University College, he resided in a street which deteriorated around him into “a slum of the worst description”, and which at one point had to be cleared by the authorities, only the professor being above reproof and left in peace. “Why should I remove?”, Beddoe reports him as saying. “I have found the world to be chiefly composed of knaves and harlots, and I would as lief live among the one as the other” (9). It was a sad indictment coming from a frustrated radical at the moment when Chartism finally failed. Political grievances, by feeding such personal bitterness, contributed by a curious dialectical process to a new scientific cynicism in the 1850s.

The “Palaeozoology” Lectures, 1853-7

The “arrest of Grant’s scientific development” (10) was a problem alluded to in the Introduction. Darwin recollects in his *Autobiography* that after Grant’s initial research at Edinburgh, he did little more in science, a problem Darwin found inexplicable (11). It has been a theme of this thesis that Grant’s scientific and social decline were inextricable, and that Lamarckism was only a proximate – and then partial – cause of his difficulty. Transformism was part of his secular strategy, itself the product of a Wakleyan ideology. His socially-levelling views reinforced by an anti-Church materialism made him unwelcome in the learned societies. So

in the background and whatever the larger issues looming there *were* overriding moral concerns over bestial transmutation, as we know from a study of Owen's scientific reaction – it was ultimately the practical implications of his political and naturalistic ideology that caused Grant's downfall. From the mid-1830s he lost institutional resources, potential funding, a platform, and publication medium. And in the early 1840s he found it difficult to publish his papers. But Grant's decline in productivity also had an economic foundation. Being unable to practise in the city, he was forced to lecture continually at UC (giving 200 lectures a year) and even then was often unable to make ends meet. Couple this with the fact that LU was primarily a Benthamite teaching factory, and that founders like Thomas Cambell saw research as an unnecessary luxury ancillary to the main aim, which was to make a profit by turning out the largest number of proprietors' sons trained in the liberal sciences. Thus Grant, like his colleagues (e.g. Sharpey, who published considerably less (12)), was pressured to teach rather than research – and when forcibly separated from the facilities provided by the ZS, his research necessarily declined. Grant diverted himself into the burning issue of medical reform, but after his 1841 address to the BMA he published little else (the BMA was itself to wither out of existence by 1844); his *Outlines* stopped abruptly at the seventh number in 1841, unfinished; the GS declined to publish his mastodon memoir shortly thereafter. From then on he published practically

nothing. Grant was never sociable; obituarists all agreed that his was “a solitary life, which was not without its privations” (13), and the MS letters I have examined confirm this (14). From the 1840s he seems to have become even more withdrawn as his finances collapsed and living conditions deteriorated.

More difficult to explain is the innate *conservatism* of the *structure* of his science over thirty years. He arrived for lectures, even in the 1860s, formally attired in swallow tail coat and choker. As students laughed at his archaic appearance, so his science was seen as a relict of the radical thirties. G. V. Poore (attended 1862-3) recalled that “his lectures remained verbally identical for many years” (15). To this E. A. Schäfer (1867-8) added:

There is no doubt that Grant as a teacher was impossible. I believe that the course of lectures which he gave in 1834 was the finest which had ever been given in English upon the subject; but then in 1867 they were the *same* lectures given in the *same* words, and science had progressed a little in the meantime ... (16).

Grant’s alienation from the Anglican community at home may partly explain the failure of his science to progress. He remained dependent upon the structure of Parisian comparative anatomy, in a period (post 1844, after Geoffroy’s death) when French science itself went into decline. Limoges has pointed out that productivity at the Muséum hit a low point between 1845 and 1855 (17). By contrast the English at the time of the Crystal Palace were beginning to consider their science

and arts as showcases to the world. In the 1850s there was a new national pride in English achievement, scientific and technological. Grant's clinging to an anachronistic Continental anatomy would have left him further out on a limb.

And instead of becoming nationally inward-looking, as so many English savants were, Grant in the 1840s and 1850s *increasingly* exploited European sources. "When my pockets permit," he told Gideon Mantell on 16 July 1850, "I devote my short summer vacation to the perusal of continental zoological museums, and tomorrow morning I embark for Rotterdam to complete my fourth and last summers examination of those of Holland" (18). Grant was a gifted linguist, an original member of the Philological Society of London (f. 1842), and fluent in all the major European languages (19). By 1847 he was studying Dutch; and two years later he was the only British representative at the convention of Dutch and Belgian philologists at Amsterdam. The "Biographical Sketch" in 1850 listed his recent itineraries, which reveal him spending more time out of France after 1845 – in Holland, Belgium, Germany, Austria, etc (20). In 1851 he was again in Holland (21); that year he was elected corresponding member of the Société de Biologie and in 1852 corresponding member of the Société Royale des Sciences of Liège (22). This is further evidence that he was, if not spurning English science as it had spurned him, at least more comfortable in a Continental milieu; something that is dramatically confirmed

by an author-citation breakdown of his Swineyan “Palaeozoology” lectures, delivered from 1853 to 1857.

Even by applying for the Swiney geology lectureship in 1852 he feared that he was placing himself “in the midst of the Philistines” (23). He was elected by the Trustees of the British Museum (the administrators of the trust under Swiney’s Deed) in succession to Carpenter. The Standing Committee was required to vet the lectures, and Grant was obliged to deposit a transcript with the Principal Librarian (24). This 257-page document in Grant’s hand, retained by the British Library, provides our only detailed knowledge of his palaeontological views. It almost certainly represents the fossil zoology summer course he had been teaching at University College since 1832, and is clearly the expanded version of the printed precis, the *General View* (1839). The manuscript is important for a number of reasons: it is possibly unique in providing detailed evidence of a materialist transformism being taught yearly in the metropolis in the decade before the *Origin*; it highlights the “generational” views a succession of University College students were exposed to; it shows the *ad hoc* expedients Grant was forced to adopt to prop up an anachronistic serial succession in the face of the savage critiques of Chambers’ *Vestiges*; and finally the lectures themselves display a cosmic pessimism, the cosmological consequences of his disillusionment with the English scientific community.

I have transcribed this MS and published an analysis in the *Archives of Natural History*, so rather than repeating my findings here I will summarize the results and refer to the published paper, which is enclosed as an appendix. The reason for my not going extensively into this MS here is that I have deliberately restricted my study-period to the radical 1830s. In the 1850s and 1860s the social and economic conditions were vastly different. These decades marked a new age of “equipoise” (25) following the demise of the radical-Chartist threat; a time when the economy was on the upturn, social conditions ameliorated, dissident literature abounded, the bourgeois takeover of science had begun by the proto x clubbers, and Lamarckism was becoming increasingly discussed, even in a GS Presidential context (26). Grant’s materialism in the 1850s reached the cynical depths despised by middle-class reformers like Huxley. For example, Grant asked in his 1857-8 examination paper, “what forces have been chiefly in operation in originating and effacing the temporary organic film of our planet?” (27). He openly quizzed students on the genetic vs. environmental causes of change (28), and was clearly unabashed about bringing his staunch reductionist-mechanist philosophy out into the open:

Although many hundreds (more than 700) of organic bodies, animal and vegetable can now be easily formed artificially from their elements in our chemical laboratories, and many living organized beings can easily be altered specifically and generically, by change of external conditions, and the whole animalization of the globe is but a chemical process, why has not a single organized species of the simplest animal or vegetable cell been yet

formed artificially from its elements, without the aid of vitality? (29).

Despite the greater toleration of naturalistic explanations at this period (arising from the reduction of natural theology's mediating power, growing belief in continuous fossil creation, and the ‘mercantile’ takeover of metropolitan science by self-proclaimed “plebeians” like Huxley (30)), Grant’s science would still have been dismissed as the product of cynical materialism. But he was no longer threatening – while in the *politicized* context of the radical thirties he might have been seen by a romantic like Owen as socially irresponsible, to a fifties’ anti-humbug idealist like Edward Forbes he was merely an “eccentric” (31).

Grant was already in his sixtieth year when he took up the Swiney appointment. The Lectureship was instituted by the reclusive Dr. George Swiney, who bequeathed £5000 in 3 per cent Consols to the British Museum in 1844 (32), with the stipulation that the lecturer should be an MD of Edinburgh University. It was tenable for five year periods, and Carpenter delivered the first series at the RI and Russell Institution from 1848-52 (33). With little competition, Grant was elected his successor (34), and delivered his yearly “Palaeozoology” courses at University College (1853), the Russell Institution (1854 and 1855), and the London Institution (1856 and 1857) (35). At UC gratis tickets were issued to Edinburgh graduates, Grant’s students and RCP members. Here and at the Russell Institution other gentlemen

were required to pay one guinea; while at the LI the lectures were free (36). Grant was “amply remunerated” (37) by the Trustees; Swiney’s stocks were in fact doing quite well – Carpenter was started off at £120 p.a. rising to £140 p.a., and Grant was earning £144 p.a. by 1856 (38).

The Continental bias of his course is evident from a statistical breakdown of his sources. Of the 22 most frequently cited authors, most were French (8) and German (7), and the rest British (4), Swiss (1), American (1), and Danish (1) (Appendix for details). Even these figures belie the importance of the British contingent: Lyell emerged preeminently as a foil – his steady-state geology taking the full force of Grant’s developmentalist attack. Nowhere was there the sort of patriotic colonialist language Secord has described for imperialist Tory geologists like Murchison (39). The flavour was defiantly Continental, and the whole read like one of the dry “muster-rolls” of Brönn and d’Orbigny, as Forbes called them (40). In other words, gentlemen geologists at home were passed over: Sedgwick and Buckland were largely ignored, and Owen was conspicuous by his absence. Interestingly, one downtrodden Englishman who ranked highly was Mantell, “arch-hater” of Owen in his own right (41), an amateur who had been welcomed into Grant’s museum and had formed an unlikely attachment to the Gower Street professor (42). (Grant sympathised with a hard-pressed confrere who was equally unappreciated by elite savants

(42)). Finally, the odd fact that the Danish speleologist Peter Lund topped the citation table may indicate a contingent aspect to these lectures: Grant was at this point learning Danish.

The most prominent feature of these lectures was an anti Lyellian motif. Grant continually denounced Lyell's "non-developmental hypothesis" as "fanciful and absurd" (44), and Lyell's *Principles* and his 1851 GS address provide Grant with a foil for his Blainvillean serialism. Of course Lyell was largely a straw man (45); but a no less necessary one for Grant, who avoided mentioning any *progressionist* alternative to his serialism. He ignored Sedgwick's and Miller's 'discontinuous progressionism' (46), objecting presumably to punctuation for theistic purposes. Nor did he allude to Owen's and Carpenter's sophisticated progressive divergence based on a von Baerian model (47). Even without crediting rival models, Grant obviously found difficulty propping up a Blainvillean model of unilinear development from monad to man. His tactic was to back-project the series model onto the fossil record, and to *recategorize* that record employing an idiosyncratic terminology, biassed towards invertebrates; one which, unlike almost all competing classifications, ignored stratigraphic criteria completely. It made no concession to the advent of mammals or men; instead, man was effectively lumped with the fishes – a peculiar arrangement and powerful example of the ideological moulding of classification:

The best attested facts of palaeontology prove that the lower classes of animals appeared before the higher, and the heated primeval surface of the earth, fitted to develop the simplest thallogene vegetable cells and the lowest protozoic infusoria in the waters, was not compatible with the development of terrestrial phanerogamic vegetation or terrestrial vertebrates. The Protozoic period here assumed must have some time have existed on the earth, and as it is made to extend from the first commencement of organic life, to the earliest appearance of terrestrial air-breathing invertebrate animals, and comprehends exclusively aquatic animals, it may be considered to embrace the first development not only of the protozoic infusoria and of all the radiated classes, but even some of the simpler aquatic helminthoid types. Considering the developmental theory as most in accordance with the established facts of palaeontology and with the known laws of nature, the Mesozoic period will embrace the development of the earliest air-breathing Annelids, all the air-breathing Articulated or only of the Entomoid classes, and not pulmonated gasteropods, but likewise all of the aquatic invertebrate classes whether helminthoid, entomoid, or molluscous breathing by branchia, which have been found between the first appearance of air-breathing terrestrial invertebrate and the first appearance of Fishes. The Cainozoic period in which we live, has commenced with the first appearance of Fishes, and includes the development [of] all the amphibian, reptilian, ornithic and mammalian types, and, unless some new and higher type appears, may extend to the extinction of life on the surface of the earth (48).

But Grant ran into consistent difficulty with this programme, and was forced to concede that neither the invertebrates nor the lowliest vertebrates showed an inexorable ascent – that in fact both invertebrates and fishes first appeared in their highest manifestations, as Chambers' critics had insisted. He therefore adopted a number of *ad hoc* expedients, explaining the effacement of pre-Silurian species by metamorphic action (due to internal heat), which left high-born fishes and reptiles as the oldest-known vertebrates.

The “Palaeozoology” MS provides unequivocal evidence of Grant’s belief in “direct generation” (49). He claimed that “All the faunae that have successively appeared upon the earth, have been necessarily continuous from one to another, and the forms succeeding have always been produced from the preceding by the normal and direct mode of generation” (50). As proof he offered the time-honoured evidence of changes induced by domestication. But he also insisted that *generic* transmutation was evidenced by metagenesis (as Owen called it). That is, he actually saw proof of transformism in the multi-generational invertebrate cycles, or “alternation of generations”, which Owen had investigated as a natural, non-bestial *alternative* to transmutation (51). (For further discussion on Grant’s image of ‘generation’ see Appendix.) Grant was now one of a number of savants discussing development or generation. Knox was invoking it in his Radical, transcendental attacks on Oxbridge, Rome, and orthodoxy (52); Baden Powell was approaching it more reverently, as a creative exercise in design logic (53); and the rationalist Herbert Spencer was back projecting his progressivist social metaphysics onto nature (54). But Grant’s image of ‘generation’ was still located in the most defiantly materialistic context. Like Chambers, Spencer, and Powell though, he did adopt a cosmic nebular backdrop by way of justification. (See Appendix for a study of his cooling earth model and its biogeographic consequences; and his attempts in later Swiney courses to correlate declining

temperatures with a rise in blood heat of evolving life.) What is interesting from the standpoint of Grant's psychology is his final misanthropic twist to classification in recognition of mankind's inexorable ice-death. Unlike cosmic optimists and social meliorists (e.g. Spencer and Chambers) Grant ended on a sour note. Continued refrigeration would lead to "successive retrograde extinction"; that is, destruction in the reverse order, starting with the least tolerant forms, the highest mammals. The process had already begun. Man's "career" is coeval with the decline rather than development of gigantic mammals, and numerous diluvial species, mammoths, megatheriums, etc., had "disappeared in his presence". On this disconsolate note he concluded his course:

With the extinction, indeed, of a few remaining elephants, giraffes, and ostriches, man will, at no distant period, form part of a pygmy fauna compared with that of his earliest epoch at present known. And while he is thus witness to a falling off of the flora and fauna of the globe, the constantly advancing refrigeration of our planet to the temperature of cosmical space, would seem to render less obvious the sources of their indefinite renovation. As animals have successively appeared upon the earth from a highly heated to a temperate condition, in a constantly ascending series of development from the protozoa to man; so in passing from the temperate to a frigid condition, the animal species may as regularly die out from man to the simplest organized cell. A metazoic period of the earth may thus be conceived to commence with the decease of the last abortive nucleus of an aquatic cell, and the zoic career of our planet thus to terminate in the same aqueous element in which life appears to have commenced (55).

Thus the final reclassification involved a new taxonomic

category, not for the reception of man, but for the period beginning with the final demise of all life on the planet. His dispassionate approach to this inevitable ice-death is as striking as his failure to add that reassuring note so characteristic of Victorian physicists. William Thomson in 1852 had first drawn attention to the threatening consequences of the second law of thermodynamics, following the run-down of the sun's energy (56). Yet he reassured readers of *Macmillan's Magazine* that the tendency to "universal rest and death" was in fact counteracted by "an overruling creative power", so that "no conclusions of dynamical science regarding the future condition of the earth can be held to give dispiriting views as to the destiny of the race of intelligent beings by which it is at present inhabited" (57). Other Christians were equally confidant. William Whewell envisaged possibly a "new creation" (58), perhaps even the Divine Society itself. Grant by contrast held out no hope of "indefinite renovation" and offered no consolation. Instead he left a bleak image of a barren earth existing for "vast cycles of ages" (59). His disenchantment with the scientific establishment reinforced by his mechanistic and anti-theistic Presbyterianism had led to a fatalistic acceptance of the final fall of man's estate.

There were other, practical, reasons for the failure of Grant's "Palaeozoology" besides this incongruous pessimism in the boom years of Pax Britannica, sterling imperialism, and social melioration. The lectures were dry, dusty, and often

no more than an interminable catalogue of fossils and sites. They were conceptually antiquated, harking back to the thirties when serialism was, if not respectable, at least more easily defensible. Most geologists and comparative anatomists now openly disputed the doctrine of inexorable ascent. Pressed by the rationalist publisher John Chapman in 1848 to explain his objection to the *Vestiges*, Owen demanded the irrelevance of transmutation “coupled with the idea of a specific direction – viz upwards” (60). By 1851 Owen and Carpenter, by modifying Geoffroyan “unity” and applying von Baer’s embryology to the fossil record, had advanced a modern theory of progressive divergence from archetypal generality (61). Owen in *his* broadside against Lyell’s steady-state geology in 1851 was able to advance a *plausible* progressionist alternative, instancing fossil lineages (e.g. of horses and rhinoceroses) that exhibited this increasing specialisation – an alternative that gave no support to any Lamarckian law of necessary ascent (62). Grant’s transformism failed to carry weight because it was not tied to a convincing model, nor could it accommodate the latest interpretations of comparative anatomy. (Darwin’s by contrast could – Carpenter recognized that Darwin’s branching evolutionary tree easily mapped onto this von Baerian model; and recently historians have come to credit such a view (63).) Grant’s science was by now looking quaintly archaic in other aspects, notably in its insistence on nebular development. The more so since many orthodox physicists, after witnessing Lord Rosse’s resolution

of a number of nebulae into star clusters, were no longer prepared to countenance nebular development (64). Indeed, they were motivated to give Rosse a beneficial hearing in order to deny the developmentalists access to this evidence.

Grant's Swiney audience was small by RI *soirée* standards, although respectable in size by comparison with the showing at UC. When the Trustees requested the attendance figures, he reported to the Principal Librarian in 1856 that he recalled "from memory, the *average* attendance at the Fossil Course to have been Fourty [sic] persons, that of the Second Course a hundred persons, and that of the Third Course sixty persons, and that of the Fourth Course a hundred and fifty persons" or an average of 87 per annum (65). He added poignantly, "Small as this number is, it is about twenty times the average attendance at my lectures on the same subject (Palaeozoology) during the last twenty years, at University College." It was this poor turn out which presumably prompted a sub-committee to recommend splitting the venue. On the election of Grant's successor, Alexander Gordon Melville, in 1857, it was decided that the courses should be run for the first three years in the Museum of Practical Geology in London, and the last two in Edinburgh (66). Others failed to enthuse over Grant's course. In 1856 he offered to continue delivering it at the RI when his Swiney tenureship lapsed, telling the Secretary:

The course consists of twelve lectures on the natural history of Extinct Animals – a subject on which I have lectured at University College since its foundation: and although I cannot say from

experience that it possesses great popular attraction, I have ever considered it as the highest branch of the studies to which I have been devoted, and I think it most becoming the high scientific character of the Royal Institution (67).

The managers response was not surprising. Grant, as a dyed-in-the-wool professional academic, was incapable of acceding to the demands for crowd-pulling dilettantism; nor was he equipped to meet those other contemporary institutional requirements, piety and utility (68). Despite the RI's Benthamite character, Grant's secular-materialism was too radical and extremist, his transformism heretical and exploded. The Secretary of the RI, Rev. John Barlow, was known to have been horrified at the prospect of Grant having anything more to do officially with the RI (69). The Managers politely turned his offer down (70).

Summary and Conclusion

Wakley in 1846 called Grant "at once the most eloquent, the most accomplished, the most self-sacrificing, and the most unrewarded man in the profession" (71). In this thesis I have tried to put this statement into social and political perspective – to explain why it took a doctrinaire radical in the embittered Reform years to appreciate Grant's eloquence, and an anti-monopolist champion of labour to understand the *ideological* value of his Parisian science. It was found necessary to employ a complex set of personal, professional, and socio-political factors to explain why a professor

showing such promise should end up “the most unrewarded man in the profession”.

The context of Wakley’s statement itself provides a clue to Grant’s predicament. The member for Finsbury was exposing corruption and privilege in the Physiology Committee at the RS, reopening sores like Newport’s Royal Medal and Roget’s plagiarism, campaigning for places in the Society for anti-aristocratic allies like Grant and Hall. He continued, talking of Grant:

We again ask, on what principle of propriety or justice is it that his name has never, during the eighteen years he has passed in the metropolis, been placed on the Council of the Royal Society, while others, every way his inferiors, have again and again been elected? Why, too, has his name never appeared on the physiological committee, when that name for honour and reputation would carry respect and esteem with it wherever science is known? The true answer to these queries would be overladen with meanness, sycophancy, and unjust neglect.

A truer answer might also embrace Grant’s own secular science and fierce reformism, themselves antithetical to the RS nobility. And ironically Wakley himself was part of Grant’s downfall. Whereas Owen, with his Peelite connections, was able to *use* his institutional privilege at the RS to patronize colonialists in return for resources (72), and thus consolidate his hold on comparative anatomy, Grant’s democratic alliances and political intransigence effectively barred him from positions of establishment power. Nor did he hanker after them while a vestige of privilege remained. Finally offered a post on the committee in the reformed society in 1849, he flatly refused it. Like Wakley, remaining

antagonistic to the newly-chartered RCS in 1843, Grant was a hardened critic for whom any compromise was betrayal: a psychological stance so clearly reflecting the unbridled class conflict in these last Chartist years.

This is not to suggest that Grant's *science* was a failure, any more than political reform was. His lectures were Parisian, progressive, and tailored to radical needs; they were enthusiastically received by liberal *Medico-Chirurgical* and radical *Lancet* readers alike. They promoted Blainvillean serialism and Geoffroyan morphology, and probably proved decisive in initiating the shift away from Cuvierian functionalism in Britain in the thirties. Thus we can assign a *historiographical* importance to Grant. He must now be ranked among Ospovat's non-Paleyean anti-teleologists (73) – indeed, judging by his influence on Carpenter and Roget, accorded a pre-eminent place as a leading diffuser of Geoffroyan morphology in Britain. But the following decade was a success for Peelite compromise when philosophical anatomy was appropriated by moderates – snatched from its materialist cradle and its radical sting removed. Just as Peelites now dominated the political machine and turned reform to their own establishmentarian ends, so Peelite savants like Owen and liberal reformers like Carpenter sanitized Grant's secular morphology and rendered Geoffroyism safe. Carpenter's surgery left it compatible with the bourgeois tastes of the naturalistic *British and Foreign* review, which supported a middle-class theology of Divine

legislation; in Owen's hands pious archetypal morphology finally became a cornerstone of cautious Peelite reform.

It was precisely because Lamarckism was tied so irrevocably to doctrinaire radicalism that it failed to transfer successfully to the capital. Grant's transformist Geoffroyan lectures were promoted in the democratic *Lancet*. Grant himself denounced monopoly, privilege, Anglican hegemony and the chartered corporations, and he recommended radical prints in class and stood on the platform of the anti-Poor Law GP's union, the BMA. The aggressive nature of extreme radicalism made transformism socially and morally repugnant to respectable reformers, and the movement's final collapse cost 'Grantism' its strongest cultural support. Even had Wakley's democratic College of Medicine been chartered in 1831, there is no guarantee that it would have sported a secular ideology as extremist as *The Lancet*'s. The "Godless College" was Lamarckism's more permanent base, but even here corporation monopoly had its influence. The RCP's failure to include comparative anatomy in its licencing examination left the LU authorities no option but to make the subject non-compulsory for the MD degree. Hence Grant's classes were small, and his plight aggravated by the financial troubles and laissez faire arrangements at the joint-stock school.

His comparative anatomy lectures in 1833 were comprehensive, philosophical, and arguably the best in the

country. He insisted on standards before profit, in face of proprietorial pressure to cheapen degrees and increase intake. The school's *embrouillés* affairs and the scandal of Pattison's sacking indirectly affected the arrangements of Grant's courses, and the failure to attract a teaching geologist led to his delivering a separate fossil zoology course from 1832. Poor attendance caused Grant to stretch himself to capacity; he lectured continually at LU and the metropolitan medical schools and provincial institutions. Being neither the circus showman nor "commercial speculator" of Brewster's declinist caricature, but retaining his 'philosophic' integrity and keeping his fees depressed below their market value, Grant exemplified all that was conscientious and self-defeating in careerist radicalism in the age of laissez faire. With no guarantee, stipend, or pension, he was left a penniless cynic.

He was initially successful at the London societies, but his materialism and uncompromising attitude made friction inevitable. Even though he lectured frequently at the Broughamite RI in the 1830s, his association with the institution withered in the 1840s. But it was at the learned societies that ideological tensions translated so disastrously into social action: his constant needling and anger at Roget disbarred him from any official position in the RS, and he was out-manoeuvred by gentlemen geologists at the GS. He had clearly seen his career interests best served by the new Bruton Street ZS, where he lectured the fellows,

sat on committees, and utilized society resources and journals in the early 1830s. Reformers like the Dissenting lawyer Bicheno had already urged a protectionist policy and the establishment of a more ‘philosophical’ society: by mid-decade radicals were openly clashing with the Tory junto, representing the game-keeping squirearchy which made up its landed backing, on crucial managerial and methodological issues. Conservative zoologists like Kirby remained suspicious of medical comparative anatomists, presumably as a result of the Lawrence fiasco. In their eyes Grant’s case would have been further jeopardized by his backing from the obnoxious *Lancet*, which remained violently antagonistic to the Tory clique on the Council led by the autocratic tax man Sabine. Sabine’s junto backed by Owen retained its control of the Council at the 1835 election by effectively ousting the dissidents. Retreating from the Society, Grant lost access to major source of funding and material.

Interpreting Grant’s science, not as a piece of decontextualized research, but as sustained by – and in turn sustaining – an extreme radical faction, we can appreciate the social and religious threat to respectable Whigs and Peelites in the troubled Reform years. Grant’s physico-mechanical reductionism provided scientific backing for Paineite thought, cutting through the cruder kinds of teleology by which the “Church and State bigots” (74) sanctioned their privileged existence. His naturalism served

to undermine Anglican hegemony in biological explanation since it could function as a judicial curb on “creative Interference” (75).

Morrell has advised us to study the scientific production of individuals associated with institutions at times of crisis to appreciate the use of knowledge in social control (76). As a socially-prominent Anglican rising fast through patronage at the elite RCS, Richard Owen makes an exemplary case study. While it is true that he was Grant’s *professional* rival – for academic posts, institutional resources, managerial responsibility, and funding – it was as a direct *political* rival, as a friend to prominent Tories, Peel’s personal advisers, and eventually the premier himself – that Owen repays close scrutiny. His politically-functioning scientific response to the moral and social threat of bestialisation is instructive precisely because his own museum came under radical flak. Grant and Wakley denounced the elitism and nepotism of RCS officials. Their attacks on the College were designed to curb Council privilege, and gain electoral rights and use of facilities for the rank-and-file. As such they demanded nothing of the College that they did not demand of parliament itself: suffrage, democratic reform, and destruction of monopoly and aristocratic privilege.

My study of Owen was designed to illustrate the social, political and institutional conditioning of a working anatomist holding respectable views in this inflammatory

period. Owen's reaction to social destabilisation was neither disingenuous nor atypical; the need for order cut across party lines, being felt to differing degrees by Peelites, Whigs, Benthamite 'legalists' and other middle-class reform groups whose demands had been to some extent met by the Reform Bill (Whigs as different as Sedgwick and Brougham agreed that Whig policy should never be such as to give radicalism an edge). Thus the broad social coalition opposed itself to the Chartists and champions of labour like Wakley and Cobbett, i.e. those who continued to voice *working class* opposition in and out of the House. By concentrating on one savant we have been able to combine a number of methodological positions (77). We have availed ourselves of Morrell and Thackray's pioneering work on the cultural uses of scientific resources by the BAAS managers who lionized Owen; MacLeod's study of the political context of institutional reform; and – since I am interested in the construction of 'good', socially-acceptable science – extensive use has been made of Barnes' and Shapin's 'instrument model' of the sociology of knowledge. Applied to Owen's morphological and palaeontological theories, this model enables us to understand how their *content* was shaped for social ends, even while remaining scientifically relevant and valid.

In this respect Owen makes a better subject for study than Lyell. Lyell's steady-state geology was never widely

accepted, and it could be considered by a sociologist like Ben-David as a paradigm case of ideological distortion (which, ironically, is how Owen viewed it). By contrast, Owen's cautious reform after the *Tamworth Manifesto* gave morphology and "speculative Palaeontology" (78) powerful new concepts which did provide a lasting foundation. I have doubted the propriety of branding Owen's anti-transmutational ideology as sterile and obstructive. Far from *retarding* scientific development – which was how it was pictured by bourgeois historiographers like Huxley – it actually served a positive heuristic value. So from the perspective of cognitive sociology, a socio-political study of Owen provides an important lesson. In the late 1840s Owen was able to build a sophisticated theory of palaeontological divergence, one onto which Darwinism was easily mapped. Yet the von Baerian embryology on which it was based was introduced in the late 1830s to crush recapitulation, serialism, and Geoffroyan 'unity' – the triple foundation supporting Grantian transmutation. I have thus been able to underscore Bowler's and Ospovat's recent internalist researches on Owen's revolutionary progressive divergence with a social and political explanation, re-connecting the subject with contemporary issues in social history.

While the procedural tools newly-honed by Lyell scholars like Bartholomew, Bowler, Ospovat, and Corsi, have proved immensely valuable in the present study, an investigation of Owen's response to Lamarckism is, I believe, even more

profitable. With Owen we need no recourse to inaccessible mental states; no ultimate ‘religious’ unit of explanation – we need not follow Bartholomew in drawing a line at Lyell’s defence of human ‘dignity’. Such a ‘psychological’ cut-off point can be circumvented by taking a contextual approach and by tackling the more encompassing aspects of social and professional conditioning. And because we possess better *contemporary* manuscript evidence of Owen’s contact with Grant and reaction to the Parisian deists, we do not have to extrapolate back using later notebook evidence. Most important of all, we can locate Owen in an *institutional* context: as an employee of the RCS he was subject to anti-materialist, patriotic, Coleridgean forces, and was patronized and groomed by the King’s own surgeons. This provides a satisfactory framework for interpreting his reaction to the besieging democratic, Lamarckian reformers. Even Owen’s own museum was dismissed by Grant as an “impediment” to the progress of liberal opinion, thus widening the gulf between the LU “lecture bazaar” and Lincoln’s Inn corporation. Owen’s response was professionally expedient: defending the integrity of his department the young social aspirant ensured the support of the eminent members of Council. But there were wider political ramifications.

Owen’s ideology manifested in the construction of esoteric palaeontological and morphological knowledge. He destroyed the structural continuity and necessary ascent demanded by

transformists. He stretched apart reptiles and mammals by disputing Geoffroy's monotreme evidence, distanced ape and man by reinterpreting chimpanzee osteology, and tackled fossil reptiles to prove the absurdity of inexorable ascent. His defeat of Geoffroy had professional pay-offs within the Lincoln's Inn medical and legal community – a community not much given to working-class sympathy – and at the aristocratic ZS, where his anti-Lamarckian *tour de force* coincided in 1835 with the conservative victory at the Council election. By denouncing French atheism he gained capital as the guardian of respectability, and was lionized by Coleridgean patriots and the clerical dons of the ancient universities. The Peelites Buckland and Whewell were particularly important for Owen's career; they appreciated the practical value of his palaeontology, which successfully toed a *progressionist* line, yet was as antipathetic to transmutation as Lyell's steady-state strategy in *Principles*. We analysed Owen's intersection with the Oxbridge dons and determined his value to the captains of the GS and BAAS on social, cultural and nationalistic grounds – a value judged by their massive 'political' allocation of funds to support his research. We concentrated on Owen's and Buckland's broad overlap of interests, and their conspiracy to out-manoeuvre Grant at the GS on the crucial question of serial progression and the Stonesfield 'opossum'. Buckland patronized Owen, stacked his lectures, and gained him the ear of the Prince Consort and Prime Minister. From these contacts Owen acquired social eminence, financial support, and royal favour (he was

presented with Sheen Lodge in Richmond Park by Queen Victoria in 1852). He successfully exploited his “Cuvierian rank” and *entrée* into Oxbridge society to press for a handsome Civil List pension from Peel in 1842. In contrast, a truly distressed comparative anatomist like Grant was still refused a pension in 1854, even though, as his University College promoters reminded Lord Aberdeen, Owen was on the list and Grant’s own pupil George Newport had just died, leaving a vacancy of £100 (79). Instead Grant was forced to rely on a stipend of £100 p.a. from the college (80), and – following Wakley’s appeal – on an annuity of £50 p.a. raised from public subscription by his pupils and colleagues in 1853 (81).

By recontextualizing biology in the 1830s and treating it from the standpoint of social injustice and radical correctives, political expediency and Peelite compromise, we can begin to *explain* why morphology developed the way it did. While it is undesirable to draw hard-and-fast class lines it seems true as a first approximation that many who were aligned with Wakley supported a materialistic Geoffroyism which served to undermine the Anglican foundations of aristocratic power; that middle-class reformers already catered for in 1832 consolidated their bourgeois gains with a naturalistic, archetypal, designful morphology; while Oxbridge Tories and the Bridgewater select adhered to older Paleyan notions and Divine creation. Generalisations like this require extreme caution. Ideal standpoints are rare; and

within each class there was tremendous leeway for doctrinal, regional, political and social variation. To get round this I have concentrated on characteristic *journals*: in particular the radical, agitating, democratic *Lancet*; reformist, middle-class *Medico-Chirurgical* and *British and Foreign* reviews; and respectable conservative *Medical Gazette* with its hatred of Wakley, radicalism, the joint-stock LU and its merchant proprietors (82). Finally, by tackling the uses to which competing scientific doctrines were put, we can employ a sociological framework of standard social, political, and class concepts, and their subordinate doctrinal, professional, and other mediating levels. By this means I have been able to hang the thesis on the most general of social premises: that Owen's manipulation of contemporary scientific doctrines, interpreted from an elitist, Coleridgean, *institutional* perspective, was a special case of the Anglican establishment's response to the democratic Chartist threat.

In short, by appreciating the full professional and social implications of the Peelite-Radical dichotomy in the turbulent thirties we can reinstate Grant's and Owen's stratagems under the rubric of social history. A full history of early Victorian morphology has yet to be written; but using the guidelines set out above it might eventually be possible to tackle it – as Berman has urged in a parallel context – “in terms of categories common to the writing of all good history: social structure, class conflict, ideology, psychological motivation and the like” (83).

Notes and References

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5. 1848-9, £82; 1849-50, £87; 1850-1, £39; 1851-2, £90; 1852-3, £33; 1853-4, £39; 1854-5, £41: Professors' Fees Book MS, UCL.
6. *The Lancet*, 2 (1850), 711.
7. R. E. Grant, *On the Present State of the Medical Profession in England* (London, Renshaw, 1841), 10.
8. C. Hall, *Memories of Marshall Hall* (London, Bentley, 1861), 69, 120.
9. Beddoe, *op. cit.* (3), 33.
10. G. V. Poore, "Robert Edmond Grant", *University College Gazette*, 2 (34) (May 1901), 190-1 (191).
11. N. Barlow, *The Autobiography of Charles Darwin* (New York, Norton, 1958), 49.
12. For Sharpey's tiny published output, consisting apparently of only fifteen items, see D. W. Taylor, "The Life and Teaching of William Sharpey (1802-1880) 'Father of Modern Physiology' in Britain", *Medical History*, 15 (1971), 126-153, 241-59 (258-9).
13. *The Lancet*, 2 (1874), 322.
14. Note his constant refusal to dinner invitations. For instance, he told his friend and supporter P. B. Ayres, who had written to congratulate him on the Swiney appointment and presumably invite him to dinner: "Dinner invitations I have very little courted through life, especially as experience has not shown me that they are the most conducive to intellectual or rational communication among cultivated minds". R. E. Grant to P. B. Ayres, 11 May 1852, MS Wellcome Institute for the History of Medicine Library. See also *Medical Times*, 2 (1974), 563-4.

15. Poore, op. cit. (10).
16. E. A. Schäfer, “William Sharpey,” *University College Gazette*, 2 (36) (October 1901), 215.
17. C. Limoges, “The Development of the Muséum d’Histoire Naturelle of Paris, c. 1800-1914”, in R. Fox and G. Weisz (eds.), *The Organization of Science and Technology in France 1808-1914* (Cambridge University Press, 1980), 211-40 (213-21).
18. R. E. Grant to G. A. Mantell, 16 July 1850, Mantell MS Papers 83, folder 44, Alexander Turnbull Library, Wellington, NZ.
19. W. Sharpey, “Obituary Notice of Dr Robert Edward [sic] Grant”, *Proc. Roy. Soc. Edin.*, 8 (1875), 486-90 (489). See the *Proceedings of the Philological Society*, 1, (1844), 2, for the list of original members.
20. BS, 691.
21. R. E. Grant to G. A. Mantell, 29 September 1851, Mantell MS Papers 83, folder 44, Alexander Turnbull Library, Wellington, NZ.
22. His diplomas are contained in an uncatalogued box marked “Robert Grant: Diplomas etc”, UCL.
23. R. E. Grant to P. B. Ayres, 11 May 1852, Wellcome Institute for the History of Medicine Library.
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25. W. L. Burn, *The Age of Equipoise: A Study of the Mid-Victorian Generation* (New York, Norton, 1965).
26. All these issues are discussed in my *Archetypes and Ancestors: Palaeontology in Victorian London 1850-1875* (London, Blond & Briggs, 1982), esp. Ch. 1.
27. Zoology Examination Papers, session 1857-8, p. 6; bound with a collection of offprints and examination papers under the title *Grant on Zoological Subjects*, College Collection DG 76, UCL.
28. Zoology examination papers, 1858-9, p. 5, in ibid.
29. Op. cit. (27), p. 1.
30. Huxley’s social allegiance is discussed in Desmond, op. cit. (26), esp. Ch. 5.

31. E. Forbes to T. H. Huxley, 16 November 1852, Imperial College MS, Vol. 16, f. 170.

32. Standing Committee Minutes, 6421, 23 March 1844; 6526, 14 December 1844: British Museum Archives. Even though the endowment was for the establishment of a *geology* lectureship, the Trustees at this late date could still appoint a Sub Committee consisting of the Archbishop of Canterbury, the Marquess of Northampton, and Mr Hallam to administer the bequest: *ibid.* 6856, 14 February 1846.

33. General Meetings, 1963-4, 3 July 1847; 2062-3, 8 July 1848; Standing Committee Minutes, 7465, 7474, 7731, 7748, 7847, 8202, 8206, 8314, 8380: British Museum Archives.

34. Three other candidates presented themselves: William Macdonald, Professor of Civil and Natural History, University of St. Andrews; David Nelson, Professor of Clinical Medicine, Queen's College, Birmingham; and Henry Jeanneret, MD: General Meetings, 2135, 8 May 1852: British Museum Archives.

35. Grant's syllabuses of the University College and London Institution courses are bound in Grant, op. cit. (27). The BM retains a syllabus of the Russell Institution course: Original Letters and Papers, 52, Jan.-June 1855, f. 101.

36. London Institution General Meetings, MS 3075 Vol. 2 "Report of the Committee of Management" 1855-6: Guildhall Library.

37. R. E. Grant to J. Barlow, 20 May 1856, Royal Institution General Archives, Box 14, File 142.

38. Standing Committee Minutes 8206, 10 May 1851, and Sub Committee Minutes 938-9, 2 December 1856: British Museum Archives.

39. J. A. Secord, "King of Siluria: Roderick Impey Murchison and the Imperial Theme in Nineteenth-Century British Geology", *Victorian Studies*, 25 (1982), 413-42.

40. E. Forbes, "Anniversary Address", *Quart. J. Geol. Soc.*, 10 (1854), xix-lxxxi (lxxxi).

41. L. Huxley, ed., *Life and Letters of Thomas Henry Huxley* (London, Macmillan, 1900), i, 93-4. M. J. Benton, "Progressionism in the 1850s: Lyell, Owen, Mantell and the Elgin fossil reptile *Leptoleuron (Telerpeton)*", *Archives of Natural History*, 11 (1982), 123-36.

42. G. A. Mantell, "The Biography of Dr. Grant", *The Lancet*,

2 (1850), 710; E. Cecil Curwen, ed., *The Journal of Gideon Mantell* (London, OUP, 1940), 282; and especially the six letters from Grant to Mantell in the Alexander Turnbull Library.

43. See Mantell, "Biography," ibid. and especially, Grant to Mantell, 20 November 1849, Alexander Turnbull Library, Mantell MS Papers 83, folder 44, where Grant sympathises: "There are many who share your opinion that British Geologists do not show a due appreciation of your long sustained and most successful labours in that most rich and most important field of inquiry As to the more tangible advantages with which fortune might be expected to reward successful labours, I believe that merit is the quality which has least concern in the distribution of her favours, and that the coxcomb of an hour is always preferred to the labour of half a century". Grant in fact refereed some of Mantell's more controversial papers (in the sense that they were considered insignificant by Owen) and as always was highly laudatory: R. E. Grant, 4 and 5 June 1850, *Referees Reports 1850-55 RR.2*, 145, 146, Royal Society.

44. R. E. Grant, Palaeozoology lectures, BL Add. MS, 31,197, f. 177.

45. P. Lawrence, "Heaven and Earth – The Relation of the Nebular Hypothesis to Geology," in W. Yourgrau and A. D. Breck, eds., *Cosmology, History, and Theology* (New York, Plenum Press, 1977), 253-81; idem., "Charles Lyell versus the Theory of Central Heat: A Reappraisal of Lyell's Place in the History of Geology," *J. Hist. Biol.*, 11 (1978); 101-28.

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47. [R. Owen], "Lyell – On Life and Successive Development", *Quarterly Review*, 89 (1851), 412-51 (449). W. B. Carpenter, *Principles of Comparative Physiology* (London, Churchill, 1854), 4th ed., 11.

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53. B. Powell, *Essays on the Spirit of the Inductive Philosophy, the Unity of Worlds, and the Philosophy of Creation* (London, Longman, 1855).

54. H. Spencer, “The Development Hypothesis”, *Essays: Scientific, Political, and Speculative* (London, Williams & Norgate, 1868 [1852]), i, 377-83. R. M. Young, “The Development of Herbert Spencer’s Concept of Evolution”, *Congres International d’Histoire des Sciences*, 11 (1) (1865), 273-8. J. D. Y. Peel, *Herbert Spencer: The Evolution of a Sociologist* (London, Heinemann, 1971).

55. Grant, “Palaeozoology”, op. cit. (44), f. 255.

56. F. A. J. L. James, “Thermodynamics and Sources of Solar Heat, 1846-1862”, *Brit. J. Hist. Sci.*, 15 (1982), 155-81 (163, 173-4).

57. W. Thompson, “On the Age of the Sun’s Heat”, *Popular Lectures and Addresses* (London, Macmillan, 1889-94), i, 349-68.

58. W. Whewell, *History of the Inductive Sciences* (London, Parker, 1857), 3rd. ed., iii, 493.

59. Grant, “Palaeozoology”, op. cit. (44), f. 23. Brooke talks of the “Evangelical stress on the depravity of man and the condescension of God...” and in Grant’s secular Presbyterianism one sees the consequence of just such a view; J. H. Brooke, “Natural Theology and the Plurality of Worlds: Observations on the Brewster-Whewell Debate”, *Ann. Sci.*, 34 (1977), 221-86 (259).

60. Rev. R. Owen, ed., *The Life of Richard Owen* (London, Murray, 1894), i, 309. Desmond, op. cit. (26), 34, 210 n27.

61. See refs. in op. cit. (47); also discussed extensively in Desmond, op. cit. (26); and D. Ospovat, “The Influence of Karl Ernst von Baer’s Embryology”, *J. Hist. Biol.*, 9 (1976), 1-28.

62. Owen, op. cit. (47).

63. Ospovat, op. cit. (61); idem, *The Development of Darwin’s Theory: Natural History, Natural Theology, and Natural Selection, 1838-1859* (Cambridge University Press, 1981).

64. M. B. Ogilvie, “Robert Chambers and the Nebular Hypothesis”, *Brit. J. Hist. Sci.*, 8 (1975), 214-32.

65. Letter Books 49, f. 127; Standing Committee Minutes, 9008, 24 May 1856; R. E. Grant to A. Panizzi, 19 May 1856, Original Letters and Papers, Vol. 54 April-Aug. 1856, f. 89: British Museum Archives.

66. Sub Committee Minutes 960, 21 April 1857, British Museum Archives.

67. R. E. Grant to J. Barlow, 3 May 1856, Royal Institution General Archives, Box 14, File 142.

68. On the institutional ethos at this time see M. Berman, *Social Change and Scientific Organization. The Royal Institution, 1799-1844* (London, Heinemann, 1978); also J. N. Hays, “Science in the City: The London Institution, 1819-1840”, *Brit. J. Hist. Sci.*, 7 (1974), 146-62; idem., “The London Lecturing Empire, 1800-50”, in I. Inkster and J. Morrell, *Metropolis and Province, Science in British Culture, 1780-1850* (Philadelphia, University of Pennsylvania Press, 1983) 91-119.

69. J. Barlow to R. Owen, n.d., BM(NH) OC Vol. 2, f. 220.

70. Managers’ Minutes, Vol. 11, f. 146, Royal Institution. Also R. E. Grant to J. Barlow, 20 May 1856, RI General Archives, Box 14, file 142.

71. *The Lancet*, 1 (1846), 418.

72. E.g. W. Brodie had shipped Owen *Dinornis* bones from New Zealand, including possibly a “seventh species”: Owen to W. Buckland 13 November 1844, BL Add MS 38,091, f. 205. Before returning to the colony Brodie determined to become a Fellow of the RS; Owen cajoled Buckland into helping, on the grounds that “it may add to the great determination which he manifests to collect and transmit specimens & information from that colony”: Owen to Buckland, 17 January 1845, BL Add MS 38,091, f. 207.

73. D. Ospovat, “Perfect Adaptation and Teleological Explanation: Approaches to the Problem of the History of Life in the Mid-Nineteenth Century”, *Studies in the History of Biology*, 2 (1978), 33-56.

74. *The Lancet*, 2 (1830-1), 689.

75. W. Buckland, *Geology and Mineralogy* (London, Pickering, 1837), i, 586.

76. J. B. Morrell, “Professors Robison and Playfair, and the *Theophobia Gallica*: Natural Philosophy, Religion and Politics in Edinburgh, 1789-1815”, *Notes and Records of the Royal Society*, 26 (1971), 43-63 (43).

77. J. Morrell and A. Thackray, *Gentlemen of Science: Early Years of the British Association for the Advancement of Science* (Oxford, Clarendon, 1981); R. M. MacLeod, “Whigs and Savants: Reflections on the Reform Movement in the Royal Society, 1830-48”, in Inkster & Morrell, op. cit. (68), 55-90; S. Shapin, “History of Science and its Sociological Reconstructions”, *History of Science*, 20 (1982), 157-211.

78. J. E. Portlock, “Anniversary Address”, *Quart. J. Geol. Soc.*, 14 (1858), xxi-clxii (lxxxii).

79. J. Wood to Sir J. Graham, 12 May 1854, BL Add. MS 43,191, f. 212; Sir J. Graham to Lord Aberdeen, 15 May 1854, BL Add MS 43,191, f. 210; and Aberdeen’s polite refusal: Lord Aberdeen to Sir J. Graham, 17 May 1854, BL Add MS, 43,191, f. 217. The relative status of “distress” among the criteria for a pension is discussed by R. MacLeod, “Science and the Civil List 1824-1914”, *Technology and Society*, 6 (1970), 47-55.

80. *The Lancet*, 2 (1850), 711.

81. The idea of a testimonial from the students was canvassed in *The Lancet* following Wakley’s biography and subsequent editorial on Grant’s lapse into “penury”: see letters from R. F. Foote and P. B. Ayers in *The Lancet*, 1 (1851), 33. Ayers (1813-1863) attended LU in 1835-6 and was frequently invited by Grant to *soirées*: see, e.g., the ALs from Grant to Ayers in the RCS and Wellcome Institute Libraries. It was Ayers who suggested that the testimonial should take the form of a life annuity: P. B. Ayers to R. Owen, 20 January 1851, BM(NH) OC Vol. 1, f. 299; and Owen even contributed five guineas: Ayers to Owen, 20 January 1851, BM(NH) OC Vol. 1, f. 304; 12 May 1851, f. 301. Carpenter was himself hard pressed but contributed two guineas, and apologized for holding back from committee work: W. B. Carpenter to P. B. Ayers, 12 June 1851, Wellcome Institute. J. S. Bowerbank was treasurer of the fund, and Webster and Ayers secretaries to the committee. A microscope and the annuity were presented in 1853 at a ceremony attended by Sharpey, Quain, Lankester, and others, with Marshall Hall in the Chair: the speeches were reported in *The Lancet*, 1 (1853), 140-2.

82. *Medical Gazette*, 7 (1830), 305.

83. M. Berman, “Hegemony and the Amateur Tradition in British Science”, *J. Social History*, 8 (1974-5), 30-50 (32).

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The Royal Society *Catalogue of Scientific Papers* lists 35 papers published by Grant. The present bibliography comprises 65 items, and includes books, lectures, and published letters.

Abbreviations

<i>EPJ</i>	<i>Edinburgh Philosophical Journal</i>
<i>ENPJ</i>	<i>Edinburgh New Philosophical Journal</i>
<i>EJS</i>	<i>Edinburgh Journal of Science</i>
<i>PZS</i>	<i>Proceedings of the Zoological Society of London</i>
<i>TLS</i>	<i>Transactions of the Zoological Society of London</i>

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“Observations on the Nature and Importance of Geology,” *ENPJ*, 1 (1826), 293-302.

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“Notice regarding the Ova of the Pontobdella muricata, Lam.”, *EJS*, 7 (1827), 160-2.

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